nebraska symposium on motivation

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nebraska symposium on motivation

1958

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INTRODUCTION

In the Introduction to the 1957 Symposium I drew a parallel between the problems which faced the earlier explorers of the American continent and the current explorers of the field of motivation in Psychology. The papers printed in this volume and read in the 1958 Symposium fit nicely into this analogy. These explorers push back the frontiers here and there, turn up some exciting new areas, lay out some new trails to previously discovered areas, and, as is the wont of Psychologists, spend long hours around their campfires discussing their methods of exploration.

The areas which seem to be drawing the attention of some of these explorers at the moment may be roughly mapped by a list of questions. Not all of those involved in this endeavor would see all of these areas as promising, and obviously these areas are somewhat overlapping and in all probability to not cover the whole territory yet to be explored. But here are some of the areas which are attracting the present explorers and some of their predecessors: Is it possible to construct a unified theory of motivation? Does drive have energizing, directional, or selective properties, or only one or two of these? Can drive be defined satisfactorily in terms of antecedent conditions; either stimuli or history of the organism? Is drive a concept which we use only as a temporary measure until we discover enough variables to make it unnecessary? Is the energy aspect of drive theory superfluous and is the drive stimulus concept sufficient? Can drive be defined in terms of concomitant events? Can drive be defined in terms of performance, or learning, or emotion, or perception? Does drive reduction have reinforcement properties? Is drive different from arousal? Is it possible to construct or translate a motivational theory in physiological terms? Where do motives come from? How do motives operate? How can motives be changed? How can motives be manipulated? How can motives be

aroused? How are motives related to onset, maintenance and termination of behavior? Do drives "sensitize" habits, or "prime" certain response tendencies? Do learned drives function differently from unlearned drives? Are there unconscious motives which can not be brought into awareness except by special means, or not at all? Are "drive" and "motive" only descriptive terms or do they have explanatory value as well?

Each one of the authors whose papers appear in the present volume addresses himself to some of these questions. Bolles suggests that drive is a concept which we invent and find useful when we know relatively little about a specific type of behavior and what controls it. He suspects that when we know more about such behavior, we have less need for such a concept. He cites as an example the manner in which Beach was able to handle the problem of male sexual behavior in the 1956 Symposium. Bolles also expresses some doubts about whether the concept of drive has any value as an explanatory concept and suspects its usefulness is confined to descriptive functions.

Estes believes it probably is not necessary to postulate an energizing factor in D. He reports some evidence to support his contention that the results of "drive discrimination" and "irrelevant drive" experiments can adequately be explained in terms of a drive stimuli concept. His report of the results of some of his own experiments and his analysis of some of the literature on experiments using deprivational conditions for drive arousal provide some provocative reading.

Spence describes a series of studies which have come out of the Iowa laboratory designed to study the role of motivational factors in learning. He reports especially on some recent experiments on the effects of drive variation on selective learning, relating the results of these experiments, and others in the literature, to the laws describing the manner in which such motivational factors determine performance in simple learning situations.

Littman raises the question of whether or not it is possible to construct a unified theory of motivation. He provides what is perhaps one of the most thorough reviews of motivational thinking--both historical and conceptual--that has appeared among these Symposium papers. His analysis leads him to the conclusion that motivation is simply a generic term and that no integrated set of laws is likely to be devised that will cover all we include in motivation. He suggests that Psychologists can more profitably devote their attention to mapping out the parameters of a variety of sub-types of motivated behavior without wasting too much energy on trying to find explanatory concepts which will cover the wide variety of behaviors which fall into this loosely defined category and which basically have relatively little in common.

Eriksen's paper probably has less content overlap with other papers in this volume than is true of any of the others. He has done a first-rate analysis of the experimental literature on unconscious processes, including a group of cleverly designed experiments performed in his own laboratory. His analysis has some very important implications for a whole host of problems currently demanding attention in Psychology, including discrimination without awareness, subliminal conditioning, subception, perceptual defense, and attention. His analysis leads him to the conclusion that there is no convincing evidence that the human organism can discriminate or differentially respond to external stimuli at a level that is more sensitive or acute than what can be obtained by verbal report.

Malmo accepts the definition of drive which attributes only energizing and not directional properties to it. He reviews the problems in defining such a concept of drive and makes a good case for attempting to define it, in part at least, in terms of concomitant rather than antecedent events. He explores the value of physiological measures as drive indicators, raising such questions as whether or not such measures apply only to situations having emotional features and whether or not physiological indicants represent a graded series of drive intensities. He reports a

series of studies, largely from his own laboratory, relating drive intensity to various physiological measures and calls attention to the fact that in very many instances there is strong evidence that this relationship is not a linear one. This finding has many implications for the interpretation of much of the literature already published in this area, and may very well provide an explanation for some rather frequently inconsistent findings in experiments from different laboratories.

In behalf of the Department of Psychology at the University of Nebraska, I once again wish to express appreciation to the participants of this 1958 Symposium for their very considerable efforts in making this Symposium a profitable training experience for our graduate students, and for their cooperation in preparing and promptly submitting their papers for publication. I also wish to express appreciation to the National Institute of Mental Health for making available the grant which makes these symposia possible. Through the cooperation of the administrative officers of the University of Nebraska and the University of Nebraska Press, we once again have the privilege of sharing these papers with our colleagues.

Marshall R. Jones

Lincoln, Nebraska
June 16, 1958

THE USEFULNESS OF THE DRIVE CONCEPT

Robert C. Bolles

Princeton University

In the earliest days of animal psychology, animals were deprived of food because, it was argued, they could then be rewarded for "working," working to obtain food. The more severe the deprivation, the more energetically the animal could be expected to behave, and the more work one could expect from it. In 1918 Woodworth introduced the word "drive" to describe this energy dimension of behavior, and this usage became quite popular (90). It was believed that just as there had been a multiplicity of instincts, so there must be a multiplicity of drives, each produced by a specific physiological disequilibrium or need, and each characterized by the suitability of specific goal objects. Proponents of the drive concept generally assumed that drives innately increase general activity and that there are more or less innate specific activity patterns aroused by goal objects. These goal objects are peculiarly capable of reinforcing instrumental learning. The ability to learn instrumental activities, it was argued, is the hallmark of the higher animals; in the course of evolution they have acquired the ability to adapt their behavior to their bodily needs. Cannon, especially, emphasized the part drives play in the self-regulatory or homeostatic nature of behavior. An animal deprived of food or water shows a rise in general activity and the required behavioral adaptation to its environment which allows the specific activity, eating or drinking, which in turn eliminates the initiating need.

The drive concept was welcomed by some who had defended the instinct doctrine because it permitted the adaptability or purposiveness of behavior to be emphasized. It is interesting to note, for example, how easily Tolman was able to translate his defense of instincts (83)

into an argument for drives (84). Perhaps others welcomed the concept because it was at once more dynamic than Watsonian Behaviorism and less vitalistic than Freud's or McDougall's psychology. Still others espoused the drive concept because of the promise of relating behavior to objective, manipulable biological conditions of the organism (64).

Whatever the historical reasons for it, there was a remarkably general acceptance of the value of the drive concept, and fairly good agreement on how drives relate to behavior. This consensus is evident in the writings of such diverse theorists as Tolman, Cannon, Dashiell, perhaps Skinner, and, of course, Hull. According to this view drives are hypothetical central states of the organism which on the one hand are due to physiological disequilibria, or needs, and which on the other hand have certain effects upon behavior. Drives were said to energize behavior, to give rise to specific activity in the presence of suitable goal objects, and also to make possible the reinforcement of instrumental responses.

The Problem of Definition

In contrast to the agreement about the general nature of drives, there has been a surprising diversity of definitions offered for the term. One reason for this variety of definitions may be that drives have had the status of "hypothetical constructs," that is, intervening variables with surplus meaning. We have tended to be so complacent in the belief that the surplus meaning refers to some universal motivating agent that we have paid little attention to the formal properties of our definitions. Some psychologists have defined drives explicitly in terms of the antecedent conditions of deprivation or stimulation. Thus, Skinner equates drives with antecedent conditions, and relates behavior to these conditions with no special reference to the motives of the organism (75). This position is essentially a denial of the utility of the drive concept, which Skinner feels may be "only a verbal device."

Others have identified bodily need as the universal motivating agent, thus providing for a functional theory of behavior which has the advantages of simplicity, parsimony, and objectivity. There is one difficulty, however, with this view: it fails to account for the facts of motivation. Some needs (e.g., some dietary deficiencies) fail to manifest themselves in behavior. More important, though, is that much of what we want to call motivated behavior occurs in the absence of any apparent physiological need. There are a number of activities like exploration, play, and social behavior (and perhaps we should include sexual behavior) that have biological utility and can be assumed to exist because of natural selection. But it is difficult to relate these types of behavior to organismic needs.

The position that at least the "primary" drives may be equated to bodily needs is also unsatisfactory, if drives are to be used to explain motivation. To be sure, motivation is correlated with need in the typical animal learning situation (or perhaps we should say confounded) but the distinction can be made, as we will see later. Further difficulties arise when different behavior indices of motivation are compared under varying degrees of deprivation; the different indices fail to yield a consistent picture (51). What has been done most commonly in practice is to put aside precise empirical formulations in favor of rough approximate laws. Thus we assume that a 23-hour food deprivation leads to a higher hunger drive than a six-hour deprivation, without making any assumptions about the magnitude of the difference. This strategy leads to what might be called "partial anchoring" on the antecedent side.

On the other hand, some psychologists have defined drive explicitly as some feature of behavior. Warden defined drive as the tendency to approach a goal object (86). P. T. Young (92) and others have retained Woodworth's original usage, defining drive as the energy of behavior. Still others have defined drive operationally in terms of activity wheel measures. Surely any definition of the

word "drive" is perfectly valid and serviceable provided only that it is employed with some universality.

The purpose of this paper is to assess the usefulness of the drive concept. There is a certain degree of usefulness, of course, to be gained from the denotative definition of drive by using the term descriptively. But Brown and Farber have pointed out that it is potentially much more fruitful to consider a term as a theoretical construct and use it as an explanatory device (10). One way to make a theoretical construct succeed as an explanatory device is to determine its functional relationships to both antecedent and behavioral terms. But the attempts to anchor theoretical constructs antecedently typically lag far behind their use as explanatory devices. This means that they are frequently used to explain the same behavioral data from which their existence is inferred. Inferring drives from behavior without reference to antecedent conditions may do no more than restate the facts that the drive was called out to explain (47).

There is another way to "validate" theoretical constructs. The problem of circularity may be overcome by postulating two or more hypothetical functional properties and relating these functional properties to certain features of behavior. The stigma of circularity may be overcome pragmatically to the degree that these functional properties can be empirically maintained.

Current interpretations of the drive concept are largely based upon Hull's postulation that drives are central states characterized by three functional properties:

1. Drives energize habits.

2. Drive reduction is the mechanism of reinforcement.

3. Associated with each drive is a characteristic stimulus with which responses may become associated (36).

Hull assumed that drives are not directly observable but, rather, are to be inferred from their hypothesized effects upon behavior. Modern drive theories emphasize either the first or the first two of these assumed functional properties. Let us now review some of the evidence pertaining to the energizing and reinforcing functions.

The Energizing Function of Drives

General Activity

In 1922 Richter found that food-deprived animals showed a higher average level of general activity than satiated animals (64). For over thirty years these and similar results have been interpreted as showing that the disturbed state of hunger "drives the organisms into activity, and the activity continues until a steady state is restored" (19, p. 141). A similar interpretation is that drives goad an animal into action, into restless seeking behavior, until a suitable goal object is found.

As early as 1928 Shirley reported an experiment which suggests a quite different interpretation (72). She recorded the hour by hour activity of animals that were regularly fed at the same time each day. Superimposed upon the typical diurnal cycle was a large increase in activity that reached a maximum just before feeding. Activity was increased even more when access to the activity wheels was limited to the one hour prior to feeding. There are a number of studies to show that when an animal is repeatedly deprived of food, activity wheel scores gradually increase for about two weeks (27, 28, 30, 31, 45, 60, 62). During this period it appears as though the hunger motivation itself were being learned.

Recently Finger, Reid, and Weasner have demonstrated quite clearly that activity cage running is a response which, like many other responses, may be rein-

forced (25). They showed that animals that were fed immediately after being in activity wheels developed significantly more activity than animals fed after a one-hour delay. In another study Reid and Finger showed that a delayed feeding group fails to increase its activity at all for thirty days (61). Since activity cage responses may be conditioned to stimuli that regularly precede feeding, then at least some of the increased activity shown in the typical situation by hungry animals would seem to be learned, rather than initiated by drive.

Campbell and Sheffield (13) have presented evidence to suggest that deprivation does not initiate activity, but only lowers the threshold for the stimuli which normally initiate activity, especially stimuli which precede food (68). Animals kept in a constant environment with little stimulation show no increase in activity unless a change of stimulation occurs. Finally, Sheffield and Campbell (68) have argued that drive, per se, has nothing to do with general activity! The activity that does occur is held to be a consequence of the behavior-energizing effect of fractional anticipatory eating responses, and these responses are assumed to be conditioned to environmental stimuli. The only function of deprivation in this view is to lower the threshold of r_G. An alternative mechanism, at a physiological level, to account for the greater responsiveness of deprived animals, is the arousal or activating function of the reticular system. One effect of deprivation may be a sensitization of the reticular system which in turn sensitizes the animal to environmental stimuli. It has been suggested by Hebb (34) and others that what we mean by drive may turn out to be just the arousal function of this system.

The relationship between drives and general activity is further complicated by the recent evidence for an autonymous "activity drive" (35, 41). A drive for activity might interfere with hunger-motivated behavior in a maze experiment, or it might facilitate bar-pressing for food in a Skinner box where the opportunity for competing responses is minimized.

Specific Activity

Specific activity is the name given here to behavior that occurs in the presence of goal objects.

The relationship between drives and one sort of specific activity, the response of approaching a goal object, has been extensively studied by Warden and his co-workers (86) using the Columbia obstruction box. They counted the number of times an animal would cross an electric grid in 20 minutes to obtain a goal object. These studies show that the frequency of approaching a goal object increases with increased deprivation, up to a point beyond which performance deteriorates. Unfortunately, the results obtained with the obstruction box are rather critically a function of the test situation. Leuba has shown that the results obtained in a five-minute observation period are strikingly different from those obtained in a 20-minute period (46). Anderson (3) and others who have investigated individual differences in relative strength of drive find that they fail to generalize across situations. Research in progress at Princeton indicates that individual differences are more apt to generalize across different drives in the same situation than across different situations with the same drive. Thus, approach behavior seems to be more a function of the situation in which it is measured, and the animal's history in that situation, than it is of deprivation conditions.

The degrees of deprivation which the Columbia studies report lead to maximum drive fail to agree with the values obtained using other behavioral indices. Neal Miller has emphasized that discrepancies often occur between different measures of what would all seem to be valid indices of motivation. Moreover, discrepancies are found when different operations for producing motivation are compared (51). There seems to be no justification for arbitrarily designating one kind of behavior as drive, as Warden et al. were careful to do (86). At least such a policy does little to increase the explanatory powers of the drive concept, since it leaves untouched

the problem of relating other kinds of behavior.

The Columbia studies also sought to determine the relative strengths of different drives. Even if we accept, for the moment, frequency of grid crossing as a measure of drive, it is difficult to interpret their results. It is difficult to see how to equate the situations to which the approach behavior applied. How much food or sexual activity corresponds to "a few laps" of water? Is the delay of reinforcement the same for all of the goal objects? Does painful electric shock have the same effect upon hungry, thirsty, and exploratory animals? What would be the effect of maintaining animals for different numbers of days on the deprivation schedules? The particular experimental conditions that were used have no claim to universality; it would seem to be quite hazardous to generalize the results beyond the conditions under which they were obtained. So much for approach behavior.

Beach finds that the drive concept is of little use in the explanation of male sexual behavior (5). He objects to it on conceptual grounds: it fails to correspond to a need. He dislikes it for empirical reasons: different behaviors that should be common indices of a sexual drive fail to agree with each other. But probably the most important reason why Beach can dispense with the concept of a sexual drive in the explanation of sexual behavior is that he understands so well the behavior itself and the factors that control it. He knows many of the hormonal and stimulus conditions that arouse particular sexual response patterns or mechanisms; the drive concept adds nothing more. Our use of the drive concept seems to depend upon our ignorance of the stimulus, neural and hormonal, conditions that control behavior.

There is a great deal of ignorance regarding the control of eating and drinking. The classical position is that a food-deprived animal will suffer from painful stomach contractions, and the joint action of this noxious stimulus and a food stimulus will initiate eating responses. Even if the site of the hunger drive is assumed to be more

central than the stomach, there are many difficulties with the classical view. It suggests no mechanisms to account for how the animal recognizes food, or how the rate of ingestion is governed, or how eating is terminated. These are all complex aspects of behavior, determined by many factors. Behavioral determinants have been found by ethologists, learning theorists, and biochemists, and in physiological and neurological laboratories. To say that the behavior is energized by a drive does little to further our understanding of it.

In this connection it is interesting to note that Neal Miller, who for years has been a leader among drive theorists, relieves the term "drive" of its theoretical connotations and only uses it descriptively when he studies the mechanisms controlling specific activity (e.g., 51, 53).

Many of the drives featured in current research are distinguished by the fact that they are not associated with any fixed consummatory response. For example, an activity drive ought to be consummated by any response. The exploratory drive is assumed to be consummated by stimulus events rather than the occurrence of some specific activity. Since these drives are generally inferred from the occurrence of the relevant activity, running, or exploring, there can be no question of the energizing effect of the drives on their own specific activities. Some question may arise, however, when these activities are compared in two or more situations. Some question may arise when two or more behavioral indices are compared. It may then be more useful to investigate the stimulus factors that control the behavior.

For the aversive drives, pain, fear, and anxiety, specific activity becomes synonymous with instrumental activity, since any response that terminates noxious stimulation is instrumental by definition and at the same time must be specific since it consummates the drive.

Instrumental Activity

For hunger and thirst it is well established that instrumental behavior is activated by deprivation. As the deprivation is made more severe (within limits), the response probability is increased, its amplitude may increase, its latency is decreased. Such findings are the reason that drives are assumed to energize behavior. Maltzman (48) has reviewed a number of studies that report a paradoxical energizing of behavior as a consequence of minimal pre-feeding, a procedure that surely reduces need and should reduce drive. A drive theorist could argue that pre-feeding does reduce drive but the effect is more than overcome by the powerful elicitation of r_G and its energizing effect. There is also evidence (discussed below) from primates that strong drives produce a deterioration of performance in complex learning tasks. But by and large the effect of deprivation is to facilitate performance.

A number of experimenters have shown that while drives enter into performance, they do not enter into habit formation (12, 43, 63, 79, 81). That is, resistance to extinction is found not to be a function of the strength of drive present during acquisition. Finan (24) came to the opposite conclusion, but his study has been attacked on methodological grounds (e.g., 63).

Heathers and Arakelian (33) have shown that the extinction of a habit is likewise not a function of the strength of drive during extinction, although, of course, the rate of responding is a function of the prevailing strength of drive (43, 59, 65, 91). Hull made these relationships explicit in the equation $E = D \times H$ (36). For those who recognize a distinction between habit acquisition (learning) and habit utilization (performance), this equation symbolizes the energizing effect of drive on instrumental responses.

Spence (78, pp. 175-179) has argued that for the aversive drives, habit strength seems to be a function

of the drive during acquisition. Spence cites three studies. In two of these, eyeblink conditioning is found to be facilitated by pairing the CS with a more intense UCS (air puff). Spence observes that in one of these experiments facilitation may be due to increased drive (a fear drive) aroused by the experimental situation. But he fails to note that in both studies the additional fear drive might also be aroused by the CS.

The third study, one by Campbell and Kraeling (11) is also open to further interpretation. These investigators measured the speed of running from an electrified alley to a goal box electrified with a lower voltage. It was found that the rate of acquisition of running speed was significantly related to the magnitude of the voltage reduction, and consequently to the amount of drive reduction. However, the variable of voltage reduction in this situation was correlated with the unconditioned speed of running on the first trial, so that the higher voltages in the alley which permit greater drive reduction also facilitate the instrumental response, making the problem easier for those animals. None of this evidence requires us to reject Hull's hypothesis that habit strength is independent of drive.

Studies are appearing now on the energizing effect of other drives (exploratory, activity, frustration) upon instrumental responses. Montgomery found that animals would learn to run to a Dashiell maze in order to explore (54). Kagan and Berkun found that animals would learn to press a bar in order to run in activity wheels (41). This type of evidence does a great deal toward relieving the problems of circularity that surrounds these drives.

The aversive drives, pain, escape, fear, and anxiety, present special problems which merit much more discussion than we can give them here. The strength of the aversive drives is usually assumed to be a monotonic, increasing function of the intensity of their initiating noxious stimuli. One very desirable feature of the pain and escape drives is the precision and ease with which

they can be manipulated and anchored antecedently.

Anxiety differs conceptually from other drives in a number of respects. For example, it serves no biological homeostatic function; anxiety is not a need. We know very little about the conditions that arouse anxiety, but it is assumed to be an acquired drive, initiated by a stimulus that has been paired with pain. In contrast to pain, which elicits hierarchies of unconditioned responses, the naive animal has no native behavioral resources for dealing with anxiety. The response to anxiety may be the same as to the original pain, but it may be extinguished and a new response learned. Schoenfeld (66) and others have questioned whether the assumption of an anxiety drive is necessary. An anxiety drive is invaluable to the drive theorist in one respect; it may be invoked to explain the energizing of behavior which cannot reasonably be attributed to the action of any other drive.

The Generalized Drive Hypothesis

Hull hypothesized that drives not only energize their "own" habits but also habits acquired under other drive conditions (36). According to this hypothesis, we should predict that a hunger-learned habit will be activated when the organism is satiated for food, but deprived of water. Webb (87) and Brandauer (8) have substantiated this prediction. There are a number of studies showing generalization of secondary reinforcement between hunger and thirst (18, 21, 22, 89). The generalized drive hypothesis can account for these results too if it is argued that the stimuli which were alleged to have secondary reinforcing power in these studies merely provided stimulus support for the instrumental behavior, and the association between stimulus and response was energized by the new drive.

A very important question has been raised by Verplanck and Hayes of whether the irrelevant drive really is irrelevant in these hunger-thirst experiments (85). They have shown that a food-deprived animal re-

duces its water consumption, and a water-deprived animal reduces its food consumption. Thus, perhaps the new drive entered into the old situation and the old drive enters into the new situation. The tacit assumption that hunger and thirst are independently manipulable and can exist in pure states is no longer tenable. The apparent activation of hunger-relevant behavior under a thirst drive may merely reflect the impossibility of "satiating" a thirsty animal for food; refusal to eat does not necessarily mean that a hunger-specific activation is not possible, or that the hunger drive stimulus is not present, or that hunger-specific secondary reinforcers will not function. These are all empirical questions and they urgently need investigation. Grice and Davis have recently reported an experiment indicating that the Webb-Brandauer results are in fact due to drive interaction rather than generalized drive (29).

Another implication of the generalized drive hypothesis is that habits should be more strongly energized if an irrelevant drive condition (one that remains unreduced) is added to the relevant drive. Of several investigators, only Kendler (42) has succeeded in showing a summation effect with hunger and thirst. A summation effect was found only for moderate degrees of irrelevant thirst drive. Kendler and others have found that the resistance to extinction of a bar-press response is not greater under 22-hour food plus water deprivation than it was with only 22-hour food deprivation. This finding suggests that a facilitory effect of an irrelevant drive only occurs when it is weaker than the relevant drive. There have been contradictory results reported on the effect of irrelevant anxiety on an instrumental running response motivated by hunger (20, 49).

There are some studies by Amsel and Siegel and their co-workers (2, 73, 74) in which animals are put in a distinctive box and shocked immediately prior to a drinking or eating test. Although these studies all seem to suffer from statistical and methodological inadequacies, they suggest that the shock experience facilitates sub-

sequent consummatory activity. It is not clear that the facilitation has anything to do with increased drive level though, since Amsel (1) and others have shown quite clearly that drinking is reduced when anxiety is introduced directly into the drinking situation. Thus it seems unlikely that the facilitation of drinking due to prior shock experience is due to persisting anxiety.

The most ambitious use of the generalized drive hypothesis is in a series of studies involving the parameter of "manifest anxiety." Individuals scoring high in manifest anxiety are assumed to have a higher inherent drive level than individuals with low manifest anxiety scores. It follows from this assumption, plus the generalized drive hypothesis, plus the assumption that drive does not enter into habit strength, that anxiety should facilitate learning of a simple problem where the correct response is prepotent. But in a problem involving competing responses, anxiety should produce interference and retard learning. The empirical confirmation of these predictions provides further evidence for the independence of drive and habit strength, but the evidence for the generalized drive hypothesis is less clear. The results of the manifest anxiety studies are compatible with the generalized drive hypothesis and also with the alternative position that the additional drive present in anxious Ss introduces additional response tendencies. Although the investigators in these studies have recognized the possibility of this alternative interpretation, they have not reported a crucial experiment (23, 80). Clearly, if the manifest anxiety experiments are to provide support for the generalized drive hypothesis, then manifest anxiety must be shown to be an indicator of an irrelevant drive. The parameter of manifest anxiety has been introduced most widely in defensive conditioning and memory-drum learning experiments where anxiety would seem to be relevant. This view is consistent with the results of Bindra et al. (7) who found that salivary conditioning is not a function of manifest anxiety, and Franks (26), who found that hunger and thirst have no effect upon defensive conditioning.

The drive theorist usually argues that behavioral decrements are due to the elicitation of interfering responses by the irrelevant drive, but that behavioral facilitation is due to the energizing function of the additional drive. This uni-directional hypothesis is much in need of independent support. If compatibility of anxiety-produced responses is really a prerequisite for facilitation, then the generalized drive hypothesis would seem to be largely superfluous.

The Reinforcing Function of Drive Reduction

The most controversial of the functional properties of drives has been Hull's postulation that drive reduction is reinforcing. Hull's law of primary reinforcement asserts that "Whenever a reaction takes place in temporal contiguity with an afferent receptor impulse . . . and this conjunction is followed by the diminution in a need (and the associate diminution in the drive, D, and in . . . s_D), there will result an increment in the tendency for that stimulus on subsequent occasions to evoke that reaction" (36, p. 71). Hull noted that, typically, when a consummatory response occurs, needs are reduced and two other events, drive reduction and s_D reduction, also occur. Having no critical evidence with which to separate the effects of these events, Hull was uncertain which of them was the crucial reinforcing agent. But he was inclined to assume that need was most basic, probably because it is a biological variable, and learning must ultimately involve the physical organism.

Other drive theorists have taken differing positions with respect to reinforcement. Brown (9), Farber (23), and Miller (50) all adhere to the position that drive reduction constitutes reinforcement, although they have expressed somewhat different interpretations of what a drive is. Brown and Farber seem to mean by drive any agent that energizes habits and whose reduction is reinforcing. Miller holds that any strong stimulus can be a drive. In this view, drive reduction is synonymous

with stimulus intensity reduction, and s_D is the crucial reinforcing agent.

Mowrer originally identified reinforcement with tension reduction, where tension may be a preparatory set or expectancy (55), or anxiety (56), as well as pain and other biological drives. Mowrer parted company with other reinforcement theorists in 1947 by denying that drive (tension) reduction reinforces anxiety and other emotional responses; instead they were assumed to be acquired according to contiguity principles, and drive reduction reinforcement was assumed to apply only to instrumental responses (57).

More recently, Mowrer has relegated tension reduction to a still lesser role (58). According to this formulation, behavior is energized by "hopes" and "fears" which are anticipatory goal responses. Drive reduction is assumed to play a part in the conditioning of goal responses, but not in the acquisition of instrumental behavior which is accounted for entirely in terms of the energizing and associative functions of drives and anticipatory reactions. Spence, whose name has been linked with reinforcement theory for many years, also has recently disavowed the drive reduction theory of reinforcement, at least with respect to instrumental behavior (78). He invokes a reinforcing function of drives only to account for classical conditioning (especially the conditioning of r_G to situational stimuli). For Seward, rs_G (r_G) and rs_D (drive) summate to produce a response strength multiplier, and both enter into selection or the associative aspect of behavior, but the drive reduction theory of reinforcement is rejected (67). Thus Spence, Mowrer, and Seward have similar one-factor drive theories according to which only an energizing function of drives is recognized and the reinforcing function is held to be unnecessary.

The use made of the drive concept by a number of current learning theorists is summarized in Table 1. A certain amount of terminological violence has been

done some of the theoretical positions by simplifying the notation in an effort to facilitate comparisons among positions. Theorists have been ranked in the table roughly in accordance with their over-all dependence upon the drive concept.

Theory		Functional effect upon Instrumental Behavior		
		Energizing	Associative	Reinforcing
Two-factor drive theories (energizing and reinforcing)	Brown (9) Farber (23)	D	s_D	$-\triangle D$
	Miller (50)	s_D	s_D r_G	$-\triangle s_D$
	Hull (36) (1943)	D	s_D r_G	$-\triangle$ Need $+\triangle r_G$
	Hull (37) (1952)	D r_G	s_D r_G	$-\triangle s_D$ $-\triangle r_G$
One-factor drive theories (energizing)	Spence (78)	D r_G	s_D r_G	*
	Mowrer (58)	s_D r_G	s_D r_G	*
	Seward (67)	s_D r_G	s_D r_G	--
Rejection of the drive concept	Contiguity theory	--	s_D r_G	--
	Sheffield (68, 70)	r_G	r_G	--

Table 1. The role of the drive concept in current theories of learning. Where:

D signifies a hypothetical energizing aspect of drive.

s_D indicates the hypothetical drive stimulus.

r_G designates any and all fractional anticipatory responses that function like drives, as well as their stimulus consequences.

$-\triangle$ symbolizes a diminution process.

* indicates that the theory implicates the drive concept for the special purpose of establishing r_D, but not for reinforcing instrumental behavior.

The crucial question for the drive reduction theory of reinforcement, and subsequently for two-factor drive theories, is whether reinforcement is dependent upon a reduction in drive (or s_D or need). The postulation that need reduction is necessary for reinforcement has not met any support when need is separated from specific (consummatory) activity or when temporal factors are considered or when direct neural intervention is effected. Although there has been a tendency to use the words "drive" and "need" interchangeably, today it is necessary to keep these terms distinct; a need-reduction theory of reinforcement is no longer tenable, although a drive-reduction theory may be.

Hull recognized that not all learning could be traced to the direct action of primary reinforcement. Hence he postulated that any stimulus which is regularly contiguous with the primary reinforcement mechanism would come to have secondary (i.e., acquired) reinforcing power (36). The causal mechanism underlying secondary reinforcement, Hull assumed, was the fractional anticipatory goal response, r_G. The postulation of two reinforcing mechanisms introduces a number of problems, as Hull noted (36, p. 99). One serious question regarding the primary-secondary dichotomy is whether both mechanisms are really necessary.

The secondary reinforcement mechanism has been emphasized by Sheffield, who has proposed that reinforcement depends upon the occurrence of specific activity and not upon drive reduction (71).[1] Sheffield has not yet made clear whether he believes specific activity has

[1]Here the distinction between specific activity (having to do with goal objects) and consummatory activity (leading to "quiescence") becomes useful. Sheffield et al. found that sexually naive male rats would learn an instrumental response in order to copulate with receptive females, although ejaculation was not permitted (71). Under these conditions the occurrence of specific activity appeared to have reinforcing power, even in the absence of a "consummatory" response, and while tension or need and undoubtedly motivation (i.e., drive) were increasing.

reinforcing power because it "rewards" or because it changes the stimulus situation. Sheffield et al. (69, 70) have also shown that the (presumably) non-need-reducing consumption of saccharine by hungry animals could serve to reinforce an instrumental approach response. This does not settle the reinforcement problem, however.

Kagan has found, with respect to sexual behavior, that both specific activity and drive reduction enter into reinforcement (40). He found that while naive male rats would learn an instrumental response to engage in specific sexual activity without consummation, learning was superior if ejaculation (i.e., drive reduction) was permitted. Moreover, Kagan found that incomplete activity lost its reinforcing power with repeated trials, which is just what would be expected if pre-ejaculatory activity was a secondary reinforcer. But secondary reinforcement is supposed to be an acquired power of stimuli associated with primary reinforcement; if Sheffield's and Kagan's animals were sexually naive, how can the existence of secondary reinforcers be expected to exist?

The same question arises in the saccharine studies. The sweet taste of saccharine makes it an incentive, one for which a rat will work, even though it fails to reduce hunger (caloric need).[2] Although rats seem to be willing to drink saccharine solutions at all times, hunger facilitates their consumption of it. But this preference yields under hunger to preference for sugar solutions and for food (15, 76, cf. 14). The critical question for drive reduction theory is whether the incentive value of saccharine falls off because it is only a secondary reinforcer. If this is the case, then its reinforcing powers should eventually become extinguished for hungry animals.

A test should give a simple answer, but Carper has

[2] The evidence for this is not clear. Saccharine does have an effect upon the internal milieu (39), but evidently fails to reduce hunger, as reflected in subsequent eating behavior (32).

found a complication. He reports that for hungry animals saccharine loses the power to reinforce an instrumental response (14), while specific activity (saccharine drinking) is facilitated more or less constantly for over a month (15, 77). Thus by the criterion of specific activity, saccharine is not a secondary reinforcer, since it fails to lose its palatability when not supported by need reduction, but evidently it is a secondary reinforcer for instrumental behavior. The question of primary-secondary reinforcement has been abandoned by some who espouse an avidity theory of motivation. In this view, there is an innate correlation between the needs of the organism, and what it likes, i.e., what it will consume. The high preference value of saccharine, then, may be considered to be a failure of the avidity mechanism, but evidence, nonetheless, for the existence of the mechanism.

Further attacks have been attempted upon the physiological basis of reinforcement. Thomson and Porter report an experiment aimed toward obtaining evidence favoring a need reduction theory (82). They found that sodium-deprived rats would learn to prefer a "subthreshold" salt solution. However, some question may be raised about the claimed exclusion of taste factors; their thresholds were obtained from normal, not salt-deficient, animals, which can discriminate (and prefer) solutions considerably more dilute than the .1% that was used. Coppock and Chambers (17) obtained learning of an instrumental response by rewarding it with intravenous glucose injections, but Chambers (16) has again raised the question of possible secondary reinforcement in the situation.

Hull et al. report an experiment that was concerned with distinguished primary and secondary reinforcement (38). They found that a dog would learn in five days to go to the fistula-feeding side of a choice point, reversing its initial preference for the sham-feeding side. It was also found that while at first the animal would sham-eat enormous quantities of food, in eight days it failed to sham-eat at all. These results certainly show that an animal can learn to satisfy its needs, but they fail to

prove that learning occurs because needs are satisfied. We do not know if the results of Hull et al. are due to need reduction or to a change in the palatability of food for sham-fed animals. The weakness of Hull's dichotomy of primary and secondary reinforcement is just the difficulties encountered in attempting to answer such questions. In his last book Hull identified the primary reinforcing agent as the "diminution of the motivational stimulus (s_D or s_G)" (37, p. 6). This change-over from need or drive to stimuli may have been an effort to make primary and secondary reinforcement more nearly parallel. Actually Hull may have been getting ready to abolish the distinction.

Miller and his students have done a series of studies, using both hunger and thirst motivation, and involving both specific and instrumental behavior indices (6, 44, 52, 53). These studies have been consistent in showing that while fistula feeding (or watering) directly into the stomach leads to drive reduction, the usual consummatory-ingestion sequence leads to greater drive reduction. A dual criterion is used to measure the reduction of drive: the reduction of consummatory activity on an immediately following test, and the weakening of an instrumental response.

Miller notes that we now have knowledge of three sets of factors which influence subsequent consummatory behavior (and, by inference, drive). These are stomach factors, head factors (specific activity), and hypothalamic factors (14). The effects in each case are so rapid that it cannot be possible for the nutritive condition of the animal, its need, to be a consideration. "The prompt effects produced by these . . . factors relieve the drive-reduction hypothesis of reinforcement from the burden of trying to account for learning in terms of the long-delayed effects of tissue needs" (53, p. 4). This would seem to provide the final severance of the drive concept from need. But a rather high price has been paid for the independence. By using criteria of drive which depend upon the energizing of habits rather

than their acquisition, Miller and his students have largely side-stepped the issue of reinforcement. Miller's evident lack of concern about reinforcement and his increasing concern with problems of motivation may reflect a doubt about the possibility of distinguishing between primary and secondary reinforcement. The evidence so far indicates that this doubt is well-founded.

Summary

The problem of drive arises because on some occasions an animal will respond in a given situation while on other occasions it fails to respond. What motivates the animal, that is, what makes it move, is commonly assumed to be some central state, called a drive, which depends upon the antecedent conditions of deprivation and stimulation. It would be very convenient if perfect correlations were to obtain between the antecedent conditions that produce drives and behavior. Unfortunately, these correlations are not perfect; the problem of drive is not that simple. While we are free to use the word "drive" descriptively to refer either to antecedent conditions or to some explicit feature of behavior, modern drive theory proposes a much greater potential usefulness for the drive concept.

According to two-factor drive theories, drives intervene, and what characterizes them, and gives value to the drive concept, is the consistent conjunction of two function properties, an energizing function and a reinforcing function. For one-factor drive theories, the drive concept carries a smaller explanatory burden than it does for two-factor theories. However, in either case the usefulness of the drive concept depends upon (1) the consistency with which its assumed functional properties can be empirically maintained and (2) the absence of these functional properties in other theoretical constructs

The evidence we have reviewed suggests several conclusions applicable to both one- and two-factor drive

theories. 1. Where little is known about the conditions that control some behavior (e.g., exploratory behavior), the inference of a causal drive does little more than restate the facts to be explained. 2. As more becomes known of the details of some behavior and the factors that control it (e.g., sexual behavior), stimulus conditions are found to play a dominant role, and the drive concept diminishes in importance. 3. The generalized drive hypothesis, which could provide a bulwark of drive theory is supported by only fragmentary evidence. Where the evidence does support the hypothesis, it is also interpretable in terms of a stimulus control principle. 4. Several theorists have found it convenient, if not necessary, to postulate an energizing function for r_G, and to give r_G a place alongside of D as a habit-strength multiplier. 5. The most crucial question here for the drive theorist is whether there is any possible way to distinguish between the energizing function of drives and a) the response-eliciting power of the stimuli that must be assumed to accompany drives and b) the energizing function of r_G.

Two-factor drive theories must face additional difficulties. The search for a universal reinforcing agent has failed to yield one. If r_G is assumed to provide a mechanism for secondary reinforcement, then again r_G and D have similar roles and, unfortunately, the distinction between primary and secondary reinforcement has not yet been operationally determined.

Finally, there is a question whether the delineation of two functional properties, energizing and reinforcing, has any basis in reality, or whether it is only a convenient fiction. Typically, the same criterion is used to determine when energizing occurs and when reinforcement has been effected. The criterion in each case is the facilitated performance of some response. Webb has reviewed a number of studies in which drive is changed abruptly, so that energizing and reinforcement are not confounded (88). As a general rule, the change of drive has little or no effect on performance, unless the drive

is reduced to zero strength.

Five years ago J. S. Brown cautioned against confusing acquired drives with acquired response tendencies (9). He had particular reference to a social drive, the "drive for money," which he argued is only a descriptive label for money-getting responses. Perhaps we should extend Brown's argument to the "primary" drives, and guard against confusing any drive with response tendencies. Thus, the "hunger drive" may be only a descriptive label for food-getting responses. Perhaps the drive concept has no more usefulness than to provide a basis for describing different kinds of behavior.

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COMMENTS ON DR. BOLLES'S PAPER

by
W. K. Estes

Distilling Dr. Bolles' interesting review rather drastically, we arrive at the conclusion that in modern behavior theory the term "drive" denotes an inferred state or condition of the organism which in turn is defined by two sets of empirical relationships. On the antecedent side, the drive state is controlled by operations of deprivation and stimulation (perhaps only intense stimulation). On the consequent side, the drive state sets the occasion for reinforcement, and intensity of the drive is directly correlated with activity.

In a few well-studied experimental situations, involving for the most part food deprivation, water deprivation, or electric shock as antecedent conditions, all of the ingredients of the operational definition are pres-

ent and "drive" can at least be used without ambiguity as a descriptive, or summarizing, concept. Its usefulness in this role breaks down, of course, when enthusiastic proponents extend usage of the term to situations in which only one of the defining relations can be identified, thereby generating such ill-endowed mutants as "exploratory drive" and even "activity drive."

If the drive concept is to serve any broader theoretical function than the descriptive one, it must be not only determined, but overdetermined by empirical relationships. We should be able to demonstrate that a given degree of drive can be produced by many alternative operations of deprivation and stimulation; and once a given degree of drive has been ascertained to exist, we should be able to make many different predictions concerning activity and reinforceability. In my own contribution to this Symposium, I have assembled numerous lines of evidence indicating that the concept of drive stimulus does serve a genuine theoretical function. However both Dr. Bolles' review and my own leave substantial doubt that the same can be said for the concept of drive as a general energizing factor.

One of Dr. Bolles' bases for questioning the usefulness of the drive concept is that intensive experimental analyses of particular forms of behavior, e.g., eating, sexual behavior, usually lead, not to explanations of the behavior in terms of broad theoretical concepts, motivational or otherwise, but to accounts in terms of detailed stimulus-response relationships. We should note in this connection, first, that we cannot reasonably expect a general theoretical concept to provide a complete account of <u>all</u> that occurs in any one situation, and, second, that there is no necessary incompatibility between developing detailed stimulus-response laws for the prediction of particular forms of behavior and simultaneously developing more general concepts expressing relationships that cut across the boundaries of particular experimental situations.

STIMULUS-RESPONSE THEORY OF DRIVE

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Since the publication of Hull's Principles of Behavior (23) in 1943, his two-factor theory of drive has moved into a steadily more dominant role in the literature of behavior theory. For a number of years, a principal motif was that of testing the two-factor conception. More recently, and especially in Spence's Behavior Theory and Conditioning (39), we find the validity of this conception no longer treated as a live issue. Attention is turned instead to such matters of detail as determining the arithmetical rule by which habit and drive interact.

Reasons for the hegemony of Hull's conception are not hard to find. Preceding formulations, insofar as they were specific at all, had viewed drive either as a particular type of stimulus (Guthrie [17]) or as a vaguely defined energizing function (46; 49, Ch. 2, 4, 5). By combining both stimulus and energizing factors into a single theory, Hull automatically enabled himself to explain far more phenomena than his nearest competitor. At the same time, Hull departed from custom even more radically by making his concepts reasonably definite and specific. This latter feature of Hull's formulation led directly to the parametric studies (8, 14, 21, 26, 31, 37, 45, 47, 48) which have done much to bring some semblance of order and structure out of a diffuse and amorphous research area.

I am sure I need not linger over the detailed manner in which Hull recast the well-weathered distinction between learning and performance. His construct E, excitatory potential, is in turn a joint function of constructs H and D, representing strength of habit and strength of drive. The construct D is defined as a

general energizing factor which combines multiplicatively with H in the equation E = D X H, familiar by now to all readers of the Journal of Experimental Psychology. The magnitude of D increases with increasing deprivation of food, water, and the like, and with intensity of such stimuli as electric shocks. Increases in D are assumed to generate corresponding increases in excitatory potential of all habits in the animal's repertoire, regardless of the previous learning history. If stimuli for two or more habits are present simultaneously, then increases in D, owing to the multiplicative property, are assumed to increase the differences among the various E's so that the highest H becomes more likely to win out over its competitors; thus increases in drive lead to stereotypy of performance. The second factor in Hull's drive theory, drive stimulus (S_D), refers to stimuli, e.g., those caused by stomach contractions or a dry throat, which vary in intensity with deprivation time, and which presumably have the same properties as any other stimuli.

Since both aspects of drive are defined in terms of unobservable states or events, it seems prudent to be especially cautious and critical in appraising the empirical evidence for them. It is obvious enough that there are variations in behavior, even systematic variations, that are not attributable to variation in observable stimulus conditions; consequently we must expect some reference to unobservables in a theory of behavior. Prevailing conceptions of drive, especially Hull's, provide some explanation for many otherwise mysterious phenomena, and they have instigated much research. But because the concepts of primary drive and drive stimulus have been useful and inspirational, it becomes all the more likely that if either aspect of current drive theory should be seriously in error, it would constitute a serious blockade in the way of further progress.

With these considerations in mind, let us briefly review the evidence for the two drive factors.

(a) Drive stimulus. In some cases the term drive refers specifically to an observable stimulus, e.g., an electric shock, a bright light, an intense noise, or to the immediate effects of such a stimulus. Concerning these instances, I am inclined to agree with a recent reviewer that "if these were all we had, it seems doubtful that the drive concept would ever have been invented."[1] What needs justification is using the same term to denote an inferred state or condition of the organism which is not known to be correlated with any such stimulus. In the case of hunger or thirst, there are associated interoceptive stimuli which can be observed by special techniques; but direct observation has neither established just how these stimuli vary with deprivation nor determined whether there are characteristic stimuli associated with all of the deprivational conditions we call drives. Thus any assumption that drive stimuli play a significant role in the determination of observed behaviors must rest primarily upon indirect evidence.

Two sources of indirect evidence are particularly relevant to this problem. The experiments of Hull (22) and Leeper (27), apparently indicating that rats can form discriminations based on different deprivational conditions (hunger and thirst), were one of the principal bases for Hull's conception of drive stimuli. Those early experiments were not adequately controlled in all respects by modern standards; however a long series of studies with progressively more refined controls (4, 20, 24, 1, 2) has built what appears to be an airtight case for the existence of discriminable cues associated with different deprivational conditions and with different degrees of deprivation. Our inferences from the studies of drive discrimination are fortified by the results of recent experiments on "irrelevant drives" which demonstrate that variation of food or water deprivation can affect learned responses exactly as does variation in intensity of a conditioned stimulus. Eninger (9), for example, has

[1] K. MacCorquodale in Annual Review of Psychology, 1955, 6, p. 30.

shown that resistance to extinction of a response trained by escape from shock is reduced by any change in the deprivational conditions (and by inference the drive stimuli) that prevailed during training; complementarily, Wickens, Hall, and Reid (44) and Levine (28) have shown that reversal of a discrimination is retarded by the continued presence of an "irrelevant" deprivational condition (and by inference the continued presence of drive stimuli to which the formerly correct response has become conditioned).

It is important to be clear just what can and what cannot be concluded from this accumulation of indirect evidence. I see no basis for reasonable doubt concerning the generalization that in any given experimental situation, discriminably different patterns of stimulation can be produced by the operations of satiation and deprivation.

Available data do not, however, suffice to apportion the drive-associated stimulation between interoceptive and proprioceptive sources. In fact, we cannot even rule out exteroceptive sources. Some writers (6, 19) have hypothesized that increased deprivation of food or water simply lowers the thresholds for external stimuli. If this hypothesis were true, drive discriminations might still be possible, but they would be specific to the particular environmental situations in which training occurred. If a discrimination between hunger and thirst, say, could be shown to exhibit no transfer from one experimental situation to another, the threshold hypothesis would have to be taken seriously, but I know of no data bearing on this possibility. In any event, throughout the remainder of this paper I shall use the term drive stimuli to denote any stimuli whose probabilities of occurrence vary as a function of deprivation time, with no commitment as to the physiological locus of the stimulation.

(b) Energizing factor. The generally positive correlations between deprivation time (for food) and "spontaneous activity" (measured by records from a running wheel or a tambour-mounted cage) have long

been a principal support of the concept of drive as an energizer. More analytical experimentation seems to have waited patiently for this rather vague concept to crystalize into the more definite form of Hull's $\underline{D}$ factor. From his formulation, Hull predicted that resistance to extinction of an instrumental response would be an increasing function of the degree of deprivation prevailing during extinction; and several studies conducted to test this prediction have yielded generally confirmatory evidence. When, for example, hungry rats are trained to make an instrumental response for food reinforcement, and extinction is carried out at varying degrees of food deprivation in different groups, the number of responses to extinction proves to be an increasing function of deprivation time over the interval from zero up to the training deprivation (7, 21, 31, 37). In Spence's recent review, even greater weight seems to be given the results of Webb (43) and Brandauer (5), who found that if extinction after such training is conducted at different levels of water deprivation while the animals are satiated for food, frequency of extinction responding is an increasing function of water deprivation. Spence's interpretation is that the $\underline{D}$ factor associated with the "irrelevant" thirst drive increases the excitatory strength of the learned habit even when the training drive has been eliminated by satiation. For quantitative evidence concerning the multiplicative property of $\underline{D}$, Spence (39) has appealed to the family of curves obtained when a measure of response strength is plotted against number of reinforcements with deprivation time as a parameter. If $\underline{D}$ combined additively with habit strength to generate excitatory strength, the curves of the family would be parallel, higher values of $\underline{D}$ simply displacing the curve further up the ordinate. If $\underline{D}$ combined multiplicatively with habit strength, the curves would diverge from a common origin to different asymptotes. Considering the empirical curve families of this sort that have been reported, one must conclude that there is very little support for the former alternative; gross deviations from parallelism are observed in almost all cases. Several studies, notably those of Perin (31), Williams (45), Ramond (33),

and Ketchel (cited in [39]) have yielded clearly divergent curves, and Spence has taken these results as confirming the idea that D is a multiplier of habit strength.

The empirical findings mentioned in this brief review are but a sample of a large and heterogeneous array which, on the whole, lines up behind the two-factor theory. The obvious conclusion would seem to be that the direction in which drive theory will develop is clearly marked, that the problems remaining for further research are merely those of working out variations on a well-established theme and filling in quantitative details.

But what if the obvious conclusion is wrong? Consider for a moment the possibility that the two-factor theory is basically unsound despite the tidy situation in which Spence's book seems to have left it. Should there be any basic weakness, can we safely assume that it will soon show up if we simply relax in the role of politely interested spectators while the busy workers of Hullian persuasion continue plotting curves and revamping functions? Perhaps so. But is there not some danger that once we have drifted into the habit of seeing our data through two-factor spectacles, we will simply respond to empirical warning signals by making slight changes in this or that assumption about the quantitative properties of D, thus forestalling indefinitely any serious attack on the ground plan of the theory?

We trust that experimental facts will lead us toward adequate theory, but they can do their part only if we permit them to speak out. If they are to tell us that a theory is unsound, we must be prepared to receive the message. One form of preparation is to assemble evidence unfavorable to the theory as carefully as we do evidence that supports it. A second is to try as hard to construct alternative theories as we do to prop up the one temporarily in favor.

Looking through Spence's review, supplemented by a sampling of current experimental journals, we find no

single fact which could be taken to refute decisively the D construct, but we do find some which are of a character to raise doubts about it.

a. Recent studies have shown that the positive relation between activity and hunger can be strongly modified by variation in conditions of reinforcement (15, 34).

b. The facilitative effects of increased deprivation upon performance of an instrumental response during both acquisition and extinction come and go through a series of apparently ample experiments (29, 31, 40, 41) in a manner which hardly suggests the operation of a simple energizing factor.

c. In Cotton's carefully controlled study of runway behavior (8), an orderly functional relation between running time and deprivation time, superficially most harmonious with D theory, virtually disappeared when running times were computed only for trials on which no clearly observable competing response occurred.

d. Experiments by Grice and Davis (16), Kendler (25), Verplanck and Hayes (42), and others upon interactions between thirst and hunger yield a complex pattern of interrelationships that is increasingly difficult to reconcile with the notion of a general energizing factor.

e. As Spence has noted (39), it is not unknown for families of curves representing response strength as a function of reinforcements with deprivation time as a parameter to display a distinct tendency toward convergence as trials continue--a most aberrant trend from the standpoint of D theory.

Finding that two-factor drive theory is not entirely adequate to handle all known facts in its intended area of application is not in itself a particularly newsworthy

accomplishment. As much can be shown almost any day for almost any theory. What is interesting is that the pattern of discrepancies suggests a possible locus for constructive theoretical activity. In the literature, both experimental and theoretical, the almost universal tendency has been to account for as many relationships as possible in terms of drive as an energizer, holding the notion of drive stimulus in reserve. The latter is brought into the picture only when absolutely necessary to handle some otherwise refractory phenomenon. But of the two factors, that of drive stimulus is the one most nearly forced upon us by experimental facts. Why, then, would it not be reasonable to try reversing the coin--to account for as many relationships as possible in terms of the apparently indispensable concept of drive stimulus, leaving the notion of a general energizing factor to be brought in only if it really turns out to be necessary?

One's immediate thought may be that "this has been tried before." But has it? Guthrie (17) has long been identified with a stimulus conception of drive, but Guthrie's views have never been developed in sufficient detail to be tested against specific experimental data. Even in Hull's formulation, drive stimulus has been the stepchild, employed only at a qualitative level while the D factor received the benefits of provisional quantification and formal incorporation into the equations defining excitatory potential.

Theory of Drive-Stimuli

The remainder of this paper will be devoted to evening up the score. My objective shall be to do as thorough a job of developing the concept of drive stimulus as Hull and Spence have done for the energizing factor.

Assumptions

1. First of all, I assume that drive stimuli are simply stimuli, with no special properties whatsoever.

Thus I shall be applying to a particular class of experiments the general stimulus theory developed in preceding papers by myself, Burke, and others (10, 11, 12, 13, 38). I shall not recapitulate previous papers here, but I shall review methods and assumptions as occasion arises to apply them in particular contexts. From this viewpoint, the essential characteristic differentiating experiments on drive from other classes of experiments is that in the former some of the important sources of stimulation are not directly observable. In order to reduce a very large task to conceivably manageable proportions, I shall deal almost exclusively with experiments upon hunger and thirst as determiners of instrumental response strength (in mammals), and within this still large class of experiments primarily with the parametric studies that have demonstrated reasonably clearcut functional relationships between response measures and degree of food or water deprivation.

2. In order to apply general stimulus theory to experiments on drive, we require explicit assumptions concerning the different sources of stimulation operative in an experimental situation. The four sources I shall distinguish are illustrated schematically in Fig. 1. The four columns in the figure represent an experimental situation at four deprivation times.[2] The row labelled CS represents experimentally controlled signals, discriminative cues, or the like, and other apparatus cues whose sampling probabilities can be assumed constant from trial to trial. The row labelled Ext. represents extraneous stimuli; these are randomized with respect to both time and experimental treatments by standard experimental designs. The row labelled Sat. represents cues, including for example those produced by distention

[2] In the next few sections I shall use the terms "deprivation" and "satiation" only with reference to some one experimentally defined drive, usually hunger; only in connection with experiments on drive interaction, the last topic to be considered, will we have occasion to consider deprivation of both food and water concurrently.

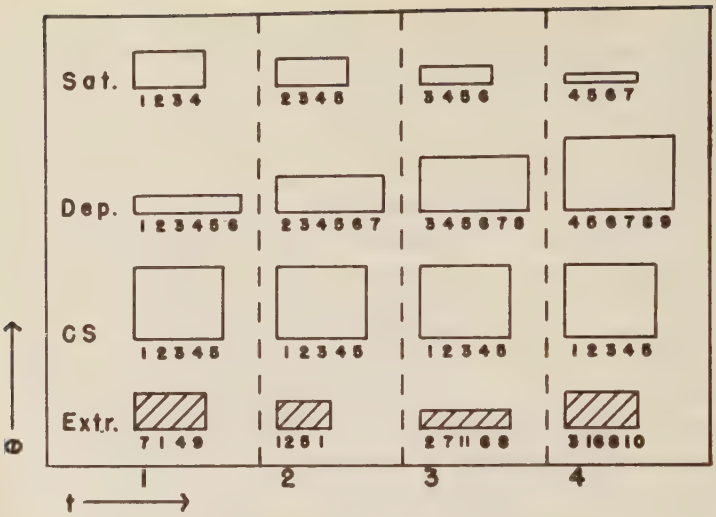

Fig. 1. Schematic representation of the four classes of stimuli assumed to be operative in any experimental situation. The classification is based on the relationship between deprivation time (t) and stimulus sampling probabilities (θ).

of the stomach, whose probabilities of occurrence decrease with deprivation time. Finally, the row labelled Dep. represents cues whose probabilities of occurrence increase with deprivation time.

The horizontal dimension of each rectangle in Fig. 1 represents the number of cues (stimulus elements) in the set, while the vertical dimension represents the average sampling probability of the elements. The area of the rectangle represents what we shall term the weight of the stimulus set; the weight is a parameter which can be evaluated experimentally and which may be taken to signify the relative importance of the given stimulus set in determining behavior.

3. Our only general assumptions concerning weights of the various stimulus sets are as follows. (a) The weight of the CS set is constant over deprivation time. (b) The weight of the Extraneous set varies randomly, but the average weight is constant over deprivation time.

(c) The weight of the Satiation set decreases with deprivation time; for simplicity in calculations, I shall take the function to be linear, as shown in the figure. (d) The weight of the Deprivation set increases with deprivation time; again for simplicity I have taken the function to be linear.[3] (e) The combined weight of Satiation and Deprivation sets together increases with deprivation time.

4. The numerals under the various rectangles in Fig. 1 signify individual cues and are intended to illustrate my assumptions concerning overlap of the sets available for sampling at different deprivation times. In the case of the CS, exactly the same set of elements is assumed available at all deprivation times. The subsets of Extraneous cues represent random samples from a population large enough so that particular individual cues will rarely recur on different trials or periods of an experiment. For Satiation cues, and similarly for Deprivation cues, we assume a sequence of partially overlapping sets, the proportion of overlap between any two sets in either sequence decreasing linearly with the time difference as illustrated in the figure. Thus as time passes following a period of eating or drinking, some of the Satiation cues produced by the consummatory activity drop out and are replaced by others, the latter being a consequence of digestion and other bodily activities that follow food or water intake with some delay; similarly the detailed makeup of the set of effective Deprivation cues is assumed to change progressively with time. The psychological significance of these assumptions is that

[3] Considering everything we know about the elementary drives, it seems unlikely to me that any single function will be found to hold generally for either Satiation or Deprivation cues; presumably the functional form can be determined empirically for any given set of conditions, if it should seem worth the trouble; for purposes of deriving qualitative and semiquantitative predictions we shall simply have to hope that the assumption of "linearity over the greater portion of the range of the independent variable" will serve here as it has in so many other contexts.

of the cues which become conditioned to a response during training at a particular deprivation time, progressively more will be replaced by unconditioned cues as we test at deprivation times further from the training value.

5. Probability of any response is assumed to be determined by all cues present at the time of testing. More specifically, probability is assumed equal to the weight of all cues connected (i.e., conditioned, associated) to the given response divided by the weight of all cues present. In Fig. 1, and others to follow, I have indicated cues connected to the reference response by white areas and cues connected to competing responses by crosshatched areas. This figure might be taken to represent an experiment in which the reference response has received extensive training at all deprivation times. Thus all of the CS, Satiation, and Deprivation sets are represented by white areas. Presumably some Extraneous cues would also have become conditioned to the response, but since these rarely recur, the Extraneous sets are nevertheless represented by crosshatched areas. Now the probability of the reference response at any one deprivation time would be given by the area of the CS, Sat., and Dep. rectangles divided by the total area of all four rectangles.

General implications of the theory

It will be apparent from Fig. 1 that in any one training situation, the effect of drive stimuli upon the asymptotic level of performance can be regarded as an amplification of the effects of the CS. In standard simple learning situations, conditions of reinforcement are such that drive cues and CS cues will become conditioned to the same responses; under these circumstances, the CS-plus-drive cues will more heavily outweigh any unconditioned extraneous cues present than would the CS alone, thus yielding a higher response probability. The magnitude of this amplifying effect will depend upon a number of conditions. Some measures of performance are more vulnerable than others to the effects of extraneous cues. When rate measures are taken, as in a Skinner box, or

response time measures, as in a runway, competing responses evoked by extraneous cues must almost invariably depress the measure of performance; but when relative frequency measures are taken, extraneous cues may have little effect. When, for example, a rat in a T-maze stops to scratch its head or explore a vagrant odor, the running time for the trial will necessarily be increased, but the choice of left vs. right alley will probably be affected, if at all, only upon trials when an extraneous cue disturbs the animal precisely at the choice point. Thus asymptotic rate of bar-pressing in a Skinner box and asymptotic running time in a runway or maze should be expected to depend strongly upon level of deprivation over a considerable range of deprivation times, while an asymptotic frequency measure in a two-choice situation should depend only weakly upon level of deprivation.[4]

If training in an instrumental situation were given at several different levels of deprivation, then one would expect, on grounds either of intuition or of general experience, that asymptotic performance would be an increasing function of deprivation time. Considering the successive columns in Fig. 1, we can readily see that this outcome is predicted by the theory; for the total white area increases as we go from left to right while the crosshatched area fluctuates around a constant value. However, we also note that if extraneous cues could be completely eliminated, the theory would no longer predict an increase in asymptotic response probability with deprivation time. The white area would still increase, but since there would be no crosshatched area to be outweighed, response probability would be constant from column to column.

It is probably impossible to eliminate all extraneous cues, partly because some of them are internal in origin

[4] This conclusion should be qualified if deprivation times become so low that response to the reinforcing stimulus is appreciably affected, a complication that will be ignored in the present paper.

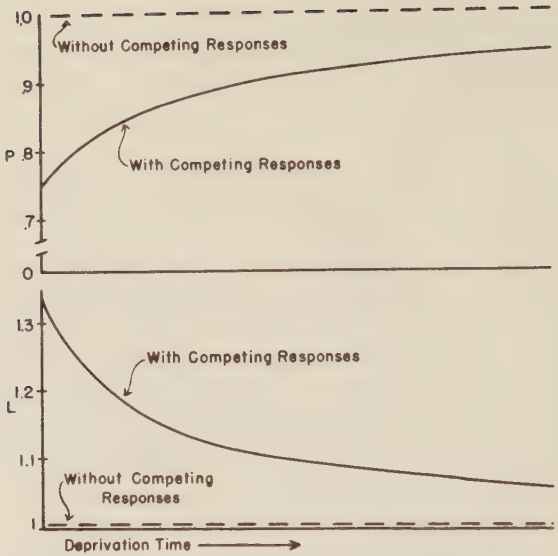

Fig. 2. Theoretical curves for Cotton's experiment.

(e.g., itches, sneezes, coughs). However, Cotton, in a doctoral research done at Indiana University a few years ago, has done the next best thing by demonstrating the effects of removing from a set of runway data all trials on which behavioral evidence indicated the presence of appreciable extraneous stimulation. Cotton trained rats on his runway at each of a series of deprivation times and then tested asymptotic running times at all deprivations. He first plotted raw mean response times, then he eliminated from the protocols all trials on which observable competing responses (face-washing, exploring cracks, etc.) occurred and replotted mean response times for the remaining trials, presumably relatively free of the effects of extraneous cues. Predictions computed from the present theory for this experiment are illustrated in Fig. 2. In the upper panel are shown theoretical response probabilities for the two conditions, and in the lower panel theoretical response times.[5] Those who have read Cotton's

[5]From earlier work on statistical learning theory (10) it is known that mean response time should be approximately equal to the reciprocal of probability.

paper (8) or who have seen his empirical functions reproduced in Spence's book will recognize the close correspondence between our predictions and Cotton's findings.

One assumption that appears tacitly in the various graphical and computational examples of this paper should be mentioned explicitly. Namely, I have assumed throughout that all drive cues (as well as the CS) are unconnected to the reference response at the beginning of an experiment and become connected only as a result of learning that may occur during the experiment. The assumption is made here because it simplifies calculations in most instances, because considering the various alternatives in each instance would unduly complicate the exposition, and because in the particular kinds of experiments considered it probably is not far from the true state of affairs. However, we should recognize that as a result of previous learning, in or out of a particular experimental situation, the population of drive cues may be preponderantly connected to the reference response previous to an experiment in some instances and preponderantly connected to competing responses in others. In any particular experimental application of the theory, one would naturally attempt to make the most reasonable assumption concerning the initial state of the various kinds of cues. Once the assumptions concerning initial conditions have been made and confirmed experimentally for any given situation, then they will be treated as a fixed part of the theory while other aspects are being tested.

Theoretical account of experiments hitherto taken as evidence for D factor

1. Activity level. Apparently one important basis for Hull's assumption of an energizing factor was the positive correlation normally observed between deprivation time and such measures of activity as operant level in a Skinner-type situation, activation of a running wheel or tambour cage when no obvious source of reinforcement is associated with responding, and asymptotic response rates or response times in instrumental learning situations. According to the present theory, however, there

is no universal or unmodifiable relation between activity and deprivation time. To the extent that behaviors which are tapped by a given measure of activity have become conditioned to drive stimuli, either during or previous to an experiment, a positive correlation between activity and deprivation is predictable by the reasoning sketched in the preceding section.[6] Even in these cases, the detailed nature of the predicted relationship differs for the two kinds of theory. On the assumption of a D factor, a high degree of deprivation should generate a greater excitatory potential than a low degree of deprivation, and for any particular measure of excitatory potential the difference between high and low deprivation groups should be constant over the period during which activity is measured. (A constant proportionality should obtain if habit strength varies over the period.) According to the stimulus theory, a higher degree of deprivation produces a higher total weight of drive stimuli which in turn tends to eliminate interruptions of the measured performance; at a low deprivation, pauses are more likely, because extraneous cues have greater relative weight, but between pauses the measure of response rate or speed should be the same as for a higher deprivation.

2. Learning curves at different deprivations. According to the stimulus theory, there are two distinct sources of differences between learning curves obtained at different degrees of deprivation. The first, already discussed, is the greater weight of the set of drive cues at higher deprivations. Since the weight differential leads to different asymptotes, at least when rate or speed measures are used, this factor taken alone would lead to a divergence of the curve family exactly as does the D factor. However, this factor does not operate alone. Referring back to Fig. 1, we recall that sampling probabilities of drive cues are higher, on the average, at higher deprivations. Consequently, conditioning of drive cues will pro-

[6] I do not mean to rule out the possibility of innate connections between drive stimuli and certain behaviors, but I do assume that, at least for mammalian forms, any such connections are modifiable by learning.

ceed more rapidly the higher the deprivation. Since this second factor operates only during the pre-asymptotic phase of learning, its effect is to produce a greater divergence of the curve family early in the learning series than would be generated by the weight differential alone. Taking the two aspects together, we can foresee that the predicted relationship between pairs of curves obtained at different deprivations is one of divergence followed by partial convergence, as shown in Fig. 3. When we are dealing with a pair of curves both obtained at relatively high deprivations, as the upper two curves in the figure, or with curves for a frequency measure which is relatively insensitive to degree of deprivation, we must predict a divergence followed by convergence to nearly equal asymptotes. When we are dealing with curves obtained at widely separated deprivations (one of which must be near the low end of the continuum), and especially if a rate or speed measure is used, the predicted picture is one of wide divergence followed by a relatively small amount of convergence.

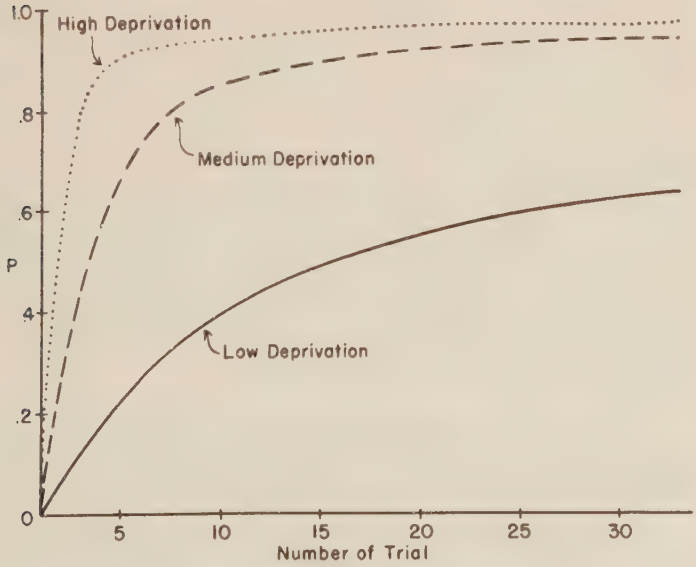

Fig. 3. Learning curves with deprivation time as a parameter.

From this analysis it appears that amount of training must be an important parameter of all studies in which measures of performance at different deprivations are compared. Generally we should expect to find maximum differences at some intermediate degree of training and smaller differences, or in some instances none, after very small or very large amounts of training. Certainly consistency of results cannot be expected among different experiments in which relationships between performance and deprivation time are tested unless measures are taken at the same point in training for all of the experiments.

A technical consideration which must be mentioned briefly is that curves plotted in terms of responses to extinction following different numbers of reinforcements, as in the well-known studies of Perin (31) and Williams (45), are not expected to show quite the same pattern as curves plotted in terms of rate or response speed. It has been shown in earlier studies of statistical learning theory (10) that if probability of a response at the beginning of extinction is equal to p, the total number of responses occurring during extinction should be approximately proportional to $\log(1/[1-p])$. So long as p values are fairly small, this quantity is almost linearly related to p, and curve families for responses to extinction should be very similar to those for the other measures considered But when p values approach unity, as they are often found to do after a sufficient number of reinforcements in simple learning situations, the log quantity diverges; under these circumstances, two curves obtained at different deprivations will be expected to resemble those of Fig. 3 in their early portions but to show no convergence as the number of reinforced trials becomes large.

3. Habit strength as a function of deprivation during acquisition and extinction. This has been one of the favorite problems for investigators of drive in relation to learning, although by no means one of the easiest. According to Hull's formulation (23), variation in D should not influence degree of learning, defined in terms

of habit strength, but only performance. Operationally, this means that on the basis of the $\bar{D}$ factor alone, performance during acquisition should depend on level of drive; performance during extinction should depend on level of drive during extinction; but performance during extinction should not depend on level of drive during acquisition. Hull's position was apparently supported by data (14) collected with the experimental design suggested by these predictions, namely one in which groups of animals receive acquisition at different deprivations and then all receive extinction at the same deprivation. The difficulty with this design, aside from the fact that it has not yielded consistent results, is that even within Hull's theory the $\bar{D}$ factor does not act alone. Drive stimuli cannot be eliminated, and within this design obviously some groups will have larger changes in stimulus conditions than others between acquisition and extinction. With the increasingly widespread use of analysis of variance methods during the postwar period, the obvious answer has been a factorial design in which the group receiving acquisition at each drive level is broken down into subgroups, one to receive extinction at each level. The recent popularity of this factorial design must be attributable to its methodological neatness, for the pattern of results yielded by it has been cloudy to say the least. Considering the main effects, extinction performance has usually been found significantly related to extinction drive, but in a few cases (40, 41) the relationship has failed to appear. On the other hand most studies have found no significant relation between extinction performance and acquisition drive, but in a very recent study by Lewis and Cotton (28) significant positive relations were demonstrated between extinction performance and both independent variables. If the various individual studies have any reliability at all, they set a stiff task indeed for a theory to deal with. We must account for a normally positive but occasionally missing correlation between extinction responding and extinction drive, for a normally insignificant but occasionally positive correlation between extinction responding and acquisition drive, for the significant interaction effects sometimes (48) but

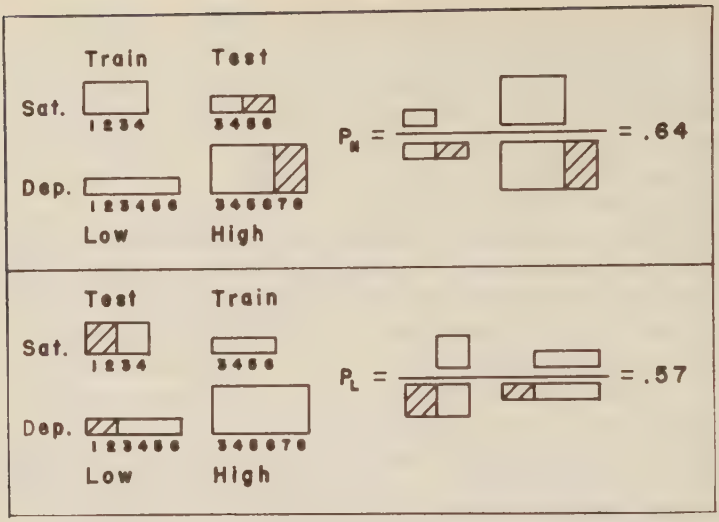

Fig. 4. Schema illustrating theoretical basis for predictions concerning generalization from training at one level of deprivation to a test at a higher (upper panel) or lower (lower panel) level of deprivation.

not always (29) found in the factorial studies. It will be of interest now to see how close our stimulus theory can come to doing all this without benefit of special ad hoc assumptions.

The basis for predictions from the stimulus theory are illustrated in Fig. 4. In the upper panel we see the theoretical result of training at a low drive and testing at a high one. Changing the level of deprivation between training and test has the effect of dropping out some of the drive cues that had become conditioned during training and replacing them with new, presumably unconditioned, ones; response probability on the test (measured, say, by response speed at the beginning of extinction) is equal to the weight of the common cues divided by the total weight of cues present on the test period. (For simplicity, I have omitted CS and Extraneous cues from this figure.) In the lower panel we see the theoretical result of training at a high drive and testing at a low one. We note that although we began with the same response prob-

ability at the end of acquisition in both cases, probability on the test is higher when the shift is from low to high deprivation than when it is from high to low deprivation. This asymmetry is a necessary consequence of the stimulus theory if, as I have indicated in the figure, the proportionate overlap between the lower and higher Deprivation sets is equal to or greater than that between the lower and higher Satiation sets; I have tentatively assumed that this relation holds generally and have used it in all computational examples shown in this paper.

Choosing parameter values that satisfy the assumptions illustrated in Fig. 4, but without so far as I know any important special properties, I have computed theoretical predictions for a hypothetical factorial experiment in which both training (to asymptote) and extinction test are conducted at four different levels of deprivation. The numerical results are shown in Table 1. The cell entries may be interpreted as response probabilities, or reciprocal latencies, at the onset of extinction under the various combinations of conditions. It may be instructive to compare the principal trends displayed by the theoretical values of Table 1 with those of impirical values shown in Table 2. The latter are probability estimates (reciprocal latencies) from a study by Yamaguchi (48).

The asymmetry of generalization to higher as compared to lower deprivations shows up clearly in both the theoretical and the empirical data, and is greater the further the pair of cells considered falls from the main diagonal. Running down the main diagonal we have a series of groups each trained and tested at the same drive, and in both tables probability is a monotonic increasing function of deprivation. If we hold test deprivation fixed at the highest value, then running down the fourth column of each table, we see that test probability increases with training deprivation. If we hold test deprivation fixed at the lowest value, then running down the first column of either table, we see that probability decreases monotonically with training deprivation. In the second and third columns, the curve of test probability

Table 1.

Factorial Experiment: Theoretical Data

Training	Test Deprivation				
Deprivation	1	2	3	4	Av.
1	.75	.85	.78	.69	.77
2	.68	.95	.87	.78	.82
3	.60	.85	.97	.88	.82
4	.52	.76	.87	.98	.78
Av.	.64	.85	.87	.83	

Table 2.

Factorial Experiment: Yamaguchi's Data

Training	Test Deprivation				
Deprivation	3	24	48	72	Av.
3	.92	.89	.95	.79	.89
24	.42	.99	1.00	.84	.81
48	.10	.88	.99	.82	.70
72	.06	.88	.89	1.00	.71
Av.	.38	.91	.96	.86	

vs. training deprivation passes through a maximum at an intermediate deprivation.

To examine the effects of varying the test deprivation for a fixed condition of training, we need only run across the rows of these tables. For the highest training deprivation, the sequence of row values increases monotonically, and rather steeply, from left to right. For all other training deprivations, the row values increase to a maximum and then decrease.

If data such as these were subjected to an analysis of variance, the main effects would be represented by variation among row means and column means. Examining these, we note that the theory and data agree on two principal points: first, the main effects attributable to variation in test deprivation are considerably larger than those attributable to training deprivation; second, both main effects, although more especially that of training deprivation, could easily go undetected, even in a very substantial experiment, if the lowest deprivations used were not very near zero. If only the three highest deprivations were used, neither the hypothetical experiment nor Yamaguchi's actual experiment would show any trace of an "energizing" effect of deprivation when analyzed as a factorial design. On the question of whether interaction effects should be readily detectable in such a design, no definite statements can be made. If the animals used in an experiment were sufficiently homogeneous, a large interaction effect could be expected; if not, the curves of response probability vs. test deprivation for different animals might have their maxima at different points along any given row of the table, and interaction effects would be obscured. Thus one important conclusion that we can draw from this analysis is purely methodological: we cannot expect to unravel the effects of acquisition and extinction drives by the mechanical application of analysis of variance designs. A conclusion of more immediate psychological import is that the pattern of empirical relationships revealed by Yamaguchi's study could quite well have arisen

from the action of stimulus variables alone and thus cannot be construed as evidence for a D factor.

Several elaborate attempts have been made to determine the detailed function relating response strength to deprivation time after training at a single deprivation (usually approximately 24 hours). The functional form prescribed by the stimulus theory is illustrated in Fig. 5. When probability or response speed is the dependent variable, the curve is negatively accelerated from the lowest deprivation up to the training value, rising steeply at first, then almost linearly over most of the range. When the dependent variable is number of responses to extinction, however, the curve becomes positively accelerated as the training deprivation is approached (assuming that response probability on the training deprivation is relatively high) for reasons we have discussed earlier. These predictions fare quite well when checked against available data. With reciprocal latency as a dependent variable, Kimble (26) obtained a curve of roughly the expected form for one to 24 hours deprivation (the

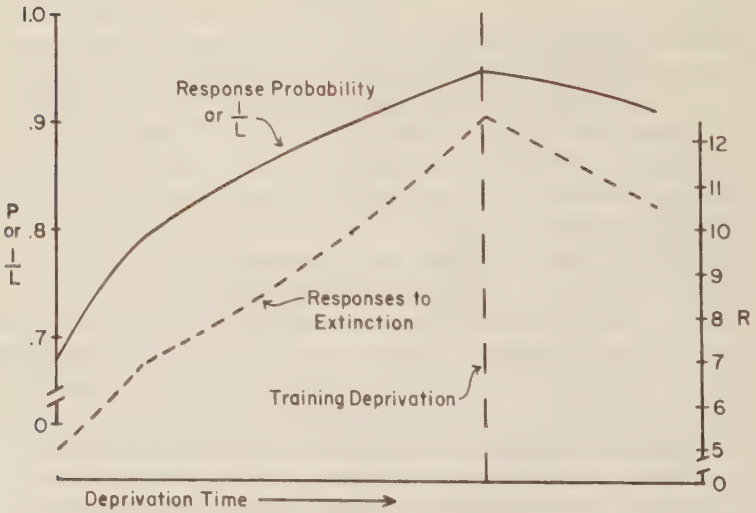

Fig. 5. Tests at various deprivations after training at a single deprivation.

latter being the training level).[7] With responses to extinction as a dependent variable, Cautela (7) obtained clear evidence of positive acceleration as the curve approached the training deprivation, then a virtually linear decline at higher deprivations. Still more pertinent is a study by Horenstein (21) in which both kinds of measures were plotted; the rate curve exhibited negative acceleration and the responses-to-extinction curve positive acceleration as the training deprivation was approached.

Zeaman and House (50) have reported an experiment on response strength at "zero" drive as a function of number of reinforcements at high drive which combines in an interesting way the relationships brought out in this section and the one preceding. When acquisition is conducted at a high drive, all drive cues present, and including of course those common to the high drive and a low drive at which tests are to be made, will have increasing probabilities of association with the reference response as the number of reinforcements increases. Consequently if tests are made on a low drive after varying numbers of reinforcements on a high drive, probability of response on the low drive should increase monotonically, and in fact should trace out a curve of virtually the same form as a learning curve obtained under reinforcement at the low drive.

4. Effects of drive upon gradients of stimulus generalization. It seems to be generally believed (18, 30) that variations in drive have strong effects upon stimulus generalization, but I have not been able to find much quantitative evidence pertaining to the presumed relationships. A study by Rosenbaum (36) with human subjects indicates that the generalization gradient of a motor response along a height dimension is elevated and flattened by the addition of an irrelevant drive, but the possibility of a converse effect was not investigated. According to

[7] Both Kimble and Saltzman and Koch (37) report little or no increase in response strength during the first half to one hour after satiation.

Hull's theory, variation in D should simply multiply any given generalization gradient through by a constant; just how, if at all, this picture should be complicated by the effects of drive stimuli, I have not been able to determine from published sources.

Relationships predicted from the present stimulus theory are illustrated in Fig. 6. The type of stimulus dimension assumed in this example is one such as wave length of light or tonal frequency, over which generalization gradients uncomplicated by changes in drive are probably essentially linear for a considerable range (18). For computational purposes, I have taken response probability to the CS at the training value equal to unity and have assumed the overlap between pairs of adjacent test stimuli to be such that the gradient, in the absence of drive cues, would fall to zero at plus and minus five steps on the abcissa. Taking into account drive cues, which would be constant over the set of tests, flattens the gradient slightly at a low deprivation and more at a high deprivation, as shown by the solid lines in the figure.

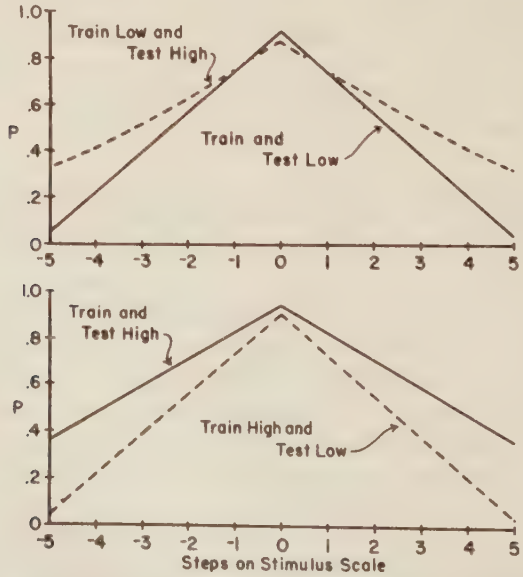

Fig. 6. Effects of deprivation upon stimulus generalization.

If after training at a low drive, the drive is increased (dashed lines in upper panel) the weight of the cues common to all test situations is increased, and the gradient is lowered and further flattened. If after training at a high drive, the drive is lowered before testing (dashed lines in the lower panel of the figure), the weight of common cues is reduced and the gradient is lowered and steepened.

5. The "energizing" effects of irrelevant drives. Before we can deal with experiments involving two or more experimentally defined drives, we require some definite assumptions concerning the presence or absence of overlap between the sets of cues associated with the various drives. Considering hunger and thirst, we must conclude from the extensive literature on drive discriminations, cited in an earlier section, that the sets of cues controlled by food and water deprivation do not wholly overlap. On the other hand, there is equally convincing evidence that these sets do partially overlap. Take, for example, a series of experiments conducted by Bishop (3) in the Indiana University laboratory. In the first experiment, rats were reinforced for bar pressing (a) 400 times with water while thirsty, (b) 800 times with water while thirsty, or (c) 400 times with water while thirsty and 400 times with food while hungry. Then all groups were given extinction first while thirsty and later while hungry, with the following results in terms of frequency of extinction responding.

Acquisition	Ext. 1 (T)	Ext. 2 (H)
400 H, 400 T	171	50
800 T	144	28
400 T	109	19

Differences between the first and third rows were significant for both periods and between the first and second rows for the second period. The fact that the group with

the additional 400 reinforcements on hunger yielded significantly more extinction responding during both periods than the group with only 400 reinforcements on thirst supports the assumption that the two deprivational conditions have common cues. A companion experiment with the following design and results (all differences being significant except those between the first and second rows of Ext. 1 and the second and third rows of Ext. 2) provides further support; the 400 additional reinforcements on thirst yield significantly more extinction responding on both drives than 400 on hunger alone.

Acquisition	Ext. 1 (H)	Ext. 2 (T)
400 H, 400 T	101	43
800 H	99	27
400 H	76	20

These findings, together with those of Porter and Miller (32), seem to warrant the assumption that the sets of drive stimuli associated with hunger and thirst have common elements throughout the range of deprivation times.

The conceptual basis for predictions generated by the present theory for the type of "irrelevant drive" experiment reported by Webb (43) and Brandauer (5) is illustrated in Fig. 7. For simplicity I have assumed the proportion of overlap between food-deprivation and water-deprivation cues to be constant at all deprivation times (and have omitted CS cues and Satiation cues, whose inclusion would not affect the predictions materially). When training is given at the highest degree of food deprivation, the reference response becomes conditioned to cues associated with both hunger and thirst. Some of the latter will be present during the test periods, the number and weight both being increasing functions of degree of water deprivation. The remainder of the drive cues present on test periods are associated with

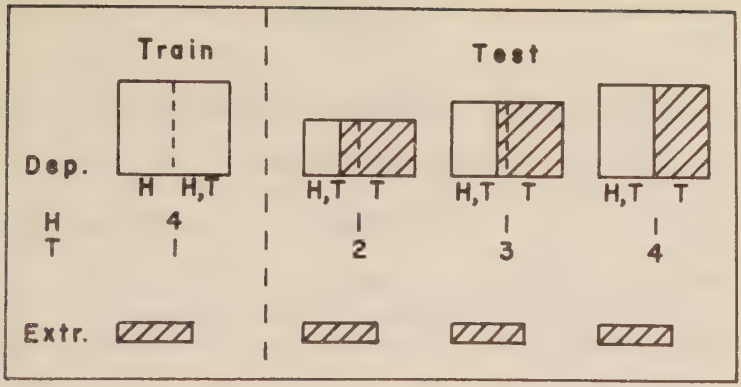

Fig. 7. Schema illustrating theoretical basis for predictions concerning "irrelevant drive" experiments of the type reported by Webb and Brandauer.

water-deprivation alone and thus have had no opportunity to become conditioned. Computing response probabilities in the usual manner, we arrive at the values shown in Table 3. We see that response probability increases with degree of water deprivation, but approaches a limit much below the response probability on the training condition--a pattern which agrees quite well with that manifested in Webb's data.

Summary

It has become progressively clearer over a period of years that accumulating data on "drive discriminations" and certain types of "irrelevant drive" manipulations virtually force upon us the concept of drive stimuli. In this paper we have made some specific, though provisional, assumptions about the quantitative properties of drive stimuli associated with such deprivational conditions as hunger and thirst. The testable consequences of these assumptions turn out to include detailed predictions of many empirical relations that have customarily been taken as evidence for the D construct of Hull and Spence. In the light of our findings, the fact that a multiplicative relation between habit strength and

Table 3.

Irrelevant Drive Experiment

Theoretical Data

	Training	Test		
Food Deprivation	High	----	----	----
Water Deprivation	----	Low	Medium	High
Response Probability	.94	.36	.43	.47

Webb's Data

	Training	Test			
Food Deprivation	22	0	0	0	0
Water Deprivation	0	0	3	12	22
Response Frequency	14.2	2.8	5.2	5.1	7.2

degree of deprivation is observed to hold over suitably restricted intervals in various experimental situations cannot be taken as strong support for the Hullian conception of a general energizing factor.

The model we have investigated in this paper must survive further tests, and may well undergo substantial revision in matters of technical detail, before it can be admitted to good standing as an explanatory theory. For the moment, we may claim a reasonable day's work if we have brought out clearly the existence of hidden riches in the hitherto overshadowed concept of drive stimuli and have put the concept in better form for experimental tests and applications.

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COMMENTS ON PROFESSOR ESTES' PAPER

by
Robert C. Bolles

While I was content in my paper to point out certain

weaknesses of the traditional drive concept, Estes has taken a much more constructive approach. He has proposed a theoretical system that promises to provide alternative explanations of many phenomena which have been taken as requiring the drive concept. Estes proposes to make drive stimuli (those stimuli that accompany deprivation) rather than a multiplicative D the crucial and central aspect of motivation. Drive stimuli are no longer to have the status of minor corollaries, to be invoked only when some data absolutely demand them.

The idea that drive stimuli are the basic motivating agents has been suggested before, by Guthrie and others, but the application of this idea to experimental data is new. Estes' Figure 1 is a Trojan horse which outwardly seems innocuous. The few simple relationships represented there should arouse little concern in the drive theorists' camp. But we have seen something of what the horse really contains: a deductive system that just might challenge the supremacy of drive theory.

Estes has also raised a number of important methodological points. For example, he notes that in the Yamaguchi type of factorially designed experiment, generalization effects and any possible energizing effects are confounded. The same confounding is apparent in most of the "irrelevant drive" investigations that have been reported. Consequently, neither of these types of study provides clear-cut evidence that an energizing function is necessary.

However, there is other evidence. Estes' theory readily accounts for Cotton's demonstration that the correlation between degree of deprivation and alley running time is largely due to the dropping out of competing responses with increasing deprivation. But drive theorists can deduce this result too if alley running is assumed to be a response that either occurs or does not occur, what Hull called an all-or-none response. The point is that Cotton's demonstration undoubtedly depends upon the prior training of all animals at all deprivation levels,

which transforms running into an all-or-none type of response. What the theory cannot account for is the correlation that exists between degree of deprivation and vigor of running when animals are trained at a single deprivation level. While highly motivated animals appear to run, slightly motivated ones only walk. Quite aside from the dimension of response probability, why should more vigorous responses be "appropriate" to higher drive conditions?

I see still more difficulty for the theory in attempting to account for a wide variety of phenomena popularly attributed to the operation of an incentive motivation factor. Estes is no doubt willing to include the stimulus consequences of r_G with other deprivation stimuli. (Actually, s_G falls into his CS category also, to the extent that r_G depends upon both deprivational and external stimulus factors.) So the drive stimulus theory may be able to handle some of the incentive motivation data.

But there are two phenomena that seem to offer special difficulties. One is the observation that animals run to high incentive goals while they walk to low incentive ones. The second is the rapidity with which animals can adapt their behavior to abrupt shifts of incentive, as demonstrated in the Crespi-Zeaman experiments and in some of the latent learning studies. These particular situations seem to offer no opportunity for any stimulus to acquire the weight necessary to control the observed behavior. On the other hand, a hypothetical incentive energizer, a K factor, makes these data easily interpretable.

While Estes is reluctant, at this time, to speculate about the physiological loci of deprivation stimuli, one very real virtue of the theory is the possibility it offers of testing such speculations. The theory contains no intervening constructs; all its terms are observable. This is true, however, only if we don't persist in inferring the existence of deprivation stimuli from the motivated behavior we observe.

One thing is clear: The associative aspects of drives may quite profitably be given much more experimental and theoretical emphasis than we have given them in the past. It remains to be seen whether such a change in emphasis will eventually enable us to discard altogether the notion of an energizing function.

BEHAVIOR THEORY AND SELECTIVE LEARNING

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For a number of years now we have been engaged at Iowa in a research program concerned with the role of motivational factors in learning. Concentrating, for the most part, on such simple learning situations as classical and instrumental conditioning, our investigations have had two main objectives. The first of these was to discover and formulate laws describing the manner in which such motivational variables as the intensity of aversive stimuli, different maintenance schedules, and different incentive values interact with each other and with other learning variables to determine performance in such simple conditioning situations. A second objective has been the formulation, on the basis of these empirical findings, of a theoretical schema that would serve to provide for the derivation and hence integration of the many specific low-order laws that are found to hold for the several response measures in the different conditioning situations.

As has been pointed out on a number of occasions (20, 21, 22), this empirical construct type of theory consists of a network of concepts which include the operationally defined experimental variables and certain auxilliary (or theoretical) concepts, sometimes referred to as intervening variables, that are connected with them by means of further definitions and postulated relations. These theoretical concepts, familiar examples of which are habit strength (H), excitatory potential (E), inhibitory potential (I_n), serve as general or abstract terms that are applicable to an organism's behavior in a variety of situations.

While the findings from any particular area of learning data may be selected as the basis for the initial formula-

tion of such an empirically based theoretical schema, Hull and I chose to develop our theories of learning on the basis of data from classical and instrumental conditioning studies. The chief reason for selecting this source was our belief that these simpler types of situations provide the best source of information for making inferences concerning the quantitative nature of the changes in the excitatory and inhibitory processes that are assumed to occur with reinforcement and nonreinforcement. That is, in the more complex, selective type of learning problem the response measure (e.g., per cent choice of the correct response) is a function of two or more competing response tendencies. In contrast the conditioning methods provide situations that reflect the changing strength of an S-R relation more or less in isolation from other S-R tendencies. Hence we have assumed that the response measures obtained in the conditioning situations reflect more directly these processes than do the selective learning situations.

Once formulated so as to be able to explain, in the sense of deduce, the known empirical findings of conditioning experiments, the theoretical model is subsequently tested by deriving implications concerning further behavior to be expected in either the same kind of experimental situations or in new learning situations. In the following presentation we shall consider examples of both kinds of such tests. Before going on to these, however, I should like to review very briefly the nature of the experimental findings with regard to the role of motivational variables in classical and instrumental conditioning and to present the relevant portions of the theoretical structure based on them.

The most extensive and best known data concerning the effect of varying the strength of an appetitional need on performance in a conditioning experiment are those provided by two studies conducted in Hull's laboratory by Perin (15) and Williams (27). As Fig. 1 shows, these investigators found that the strength of a lever-pressing response, measured in terms of resistance to extinction

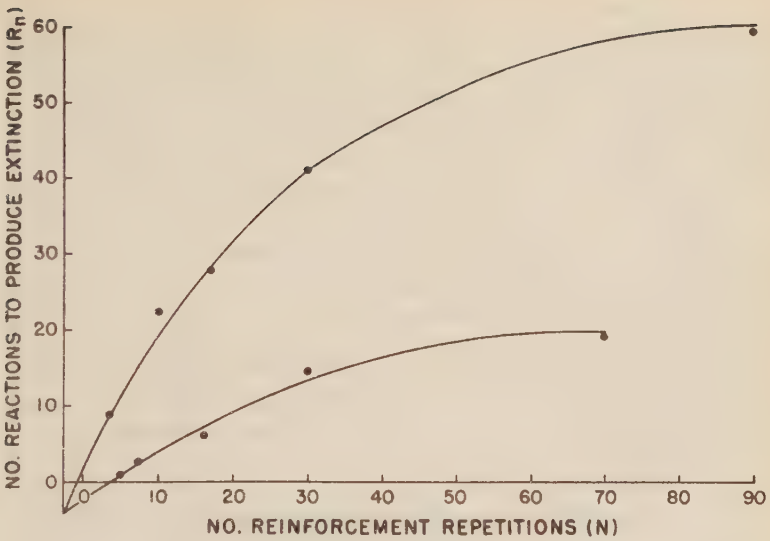

Fig. 1. Data from Perin (15) and Williams (27) showing the relation between response strength (resistance to extinction) and (a) number of previous reinforcements, (b) and time of food deprivation (T_d).

(shown on ordinate), is a function both of the number (N) of reinforced trials (shown on abscissa) and the deprivation period (T_d). Thus the curve for the 22-hour hunger group (upper curve) may be seen to rise considerably above that for the three-hour group. The fact, moreover, that the curves for the two hunger levels diverge from each other with training suggests that the two experimental variables, N and T_d, combine in a multiplicative rather than an additive fashion to determine response strength. Or, in other words, these data suggest that the joint or combination law relating R with N and T_d is of the type shown in the first of the following two equations:

$$R = (fN) \times (fT_d)$$

$$R = (fN) + (fT_d)$$

If the law had been a simple additive one as represented in the second equation, the two curves would have

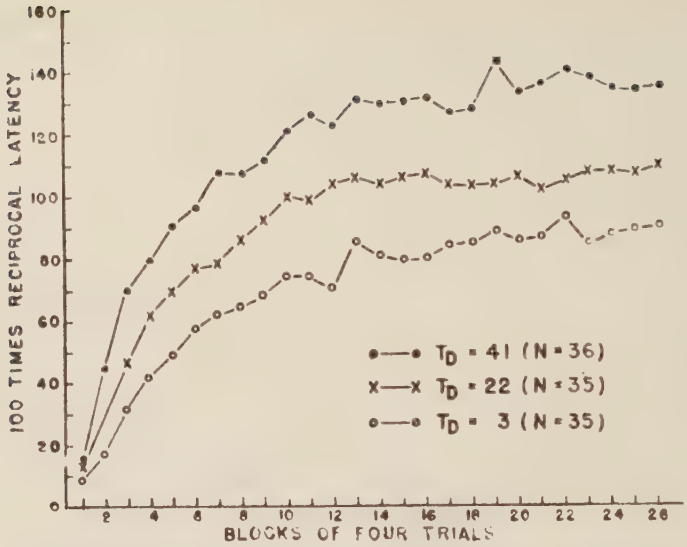

Fig. 2. Data from Davenport (7). The three curves represent speed of responding during the acquisition of a simple instrumental response under three different drive levels.

been parallel rather than diverging from each another.

Since these original investigations, we have carried out a number of thesis studies involving speed of response evocation or running as the performance measure which have provided further data bearing on the nature of this combination law. As may be seen from Fig. 2, which presents the results of one of these studies (7) that has not as yet been published, the performance curve for different hunger levels again shows divergence and thus corroborates the inference that the combination law for N and T_d (in the case of food) is a multiplicative one. The findings published by Ramond in two further studies (17, 18) also point definitely to such a multiplicative relation.

Turning next to studies employing aversive forms of motivation, the evidence with regard to the manner in which such motivational variables combine with the learning

variable in determining behavior is somewhat inconsistent. This is especially the case in instrumental escape conditioning in which strong shock stimuli have been used. Thus in three experiments of this type, one gave diverging, one parallel, and one converging learning curves for different shock levels (1, 4, 11). All three studies employed response speed as the performance measure. It is possible that one runs into a ceiling effect or physiological limit in the case of this behavioral measure. Such a limit would, of course, tend to preclude diverging performance curves with training at very high motivational levels. In this connection it is worth noting that the one study that did obtain diverging curves employed relatively weak shocks, whereas the experiment in which converging curves were obtained used extremely strong shock values. It is, of course, not uncommon in science for a law to hold only within a certain range of the values of the variables. Psychologists have not always been sufficiently aware of this possibility and need to give more attention to it.

Fortunately, in the area of classical aversive conditioning we have a considerable number of studies in which performance curves have been obtained at different levels of motivation, i.e., with different intensities of the unconditioned stimulus (S_u). In my Silliman Lectures (20) the findings of four such studies, three of them from our own laboratory, were presented. In every instance the performance curves in terms of the frequency of CR's in successive blocks of ten trials diverged from one another as a function of the number of conditioning trials. Fig. 3 presents some new data we have obtained. This graph presents the conditioning curves of four groups of subjects that were run in different investigations recently completed in our laboratory. Except for the intensity of the air puff that was used as the unconditioned stimulus, the experimental conditions (CS, CS-UCS interval, etc.) were essentially the same. For purposes of this graph the frequency measure was transformed into what are called E_L values by means of the normal probability table (see 20, p. 104 ff.). It should be understood that just as in the case

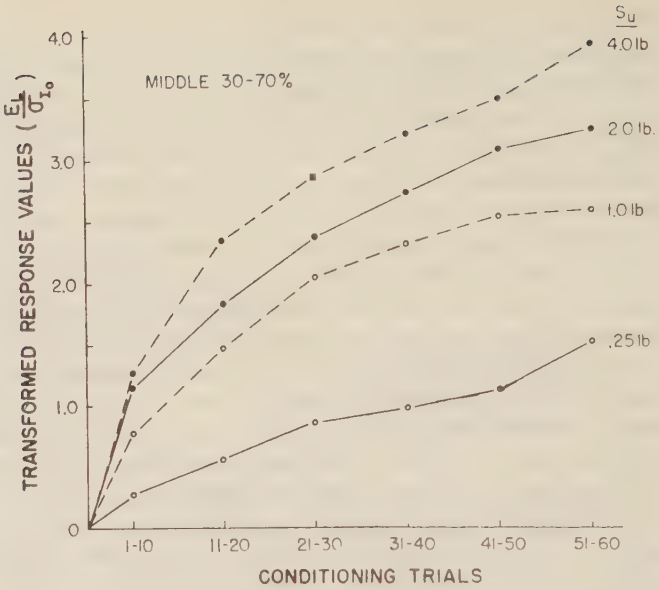

Fig. 3. Performance during acquisition of eyelid conditioned responses as a function of the intensity of the UCS.

of logarithmic and reciprocal transformations, this transformed measure is purely empirical.

Reverting to Fig. 3, it is clearly evident that the curves exhibit an increasing divergence with conditioning trials, thus corroborating the multiplicative law found in the case of appetitional motivation. Perhaps it is worthwhile pointing out here that it is necessary to take certain precautions in connection with such data. Thus the curves have been cut off at 60 trials even though the maximum level of conditioning has not been reached. The reason for doing this is that the frequency measure has a ceiling, i.e., 100 per cent. Once performance reaches this point this measure is not able to reflect any further increase that the response tendency may be developing. The present curves were cut off at 60 trials even though further trials were given all groups, because beyond this point many of the subjects, particularly in the strong-puff groups

were responding with a frequency of 100 per cent. Up to 60 trials very few subjects reached this level in any block of ten trials. However, in the 4.0 lb. group there were few such subjects only because we used the data from the middle 40 per cent of the group. Many of the subjects in the upper 30 per cent of this strong-puff group were responding at a 100 per cent level by the second or third block of trials. Obviously, inclusion of the data of these subjects would have distorted the performance curve of this group as compared with the weaker-puff groups, in which asymptotic performance was not attained even in 60 trials.

While the limitations of time will not permit a treatment of the implications for selective learning of the incentive motivation portion of our theory, I should like, nevertheless, to mention briefly the findings of the studies that have investigated these variables in conditioning experiments. The experimental variable manipulated in these studies, all of which are of the instrumental reward type, has been some property of the incentive or reward agent, such as its size, number, period of exposure, and, in the case of such things as a water solution of sucrose, the strength of its concentration. Very briefly and in general, these investigations have shown that animal subjects will run down a straight alley faster, or depress a lever at a higher rate, the greater the magnitude, number, concentration, etc., of the reinforcing agent. Again what little data there are suggest that performance curves for different reward magnitudes (etc.) exhibit the same type of interaction with training trials that was found in the drive studies. Thus the evidence, such as it is, suggests that the combination law relating response strength to magnitude of reward and number of trials is also of the multiplicative kind, i.e., $R = (fN) \times (fW_g)$ where W_g refers to the incentive variable.

This completes our review of the experimental findings bearing on the role of motivational variables in conditioning experiments. Perhaps it should be pointed out here that there are still many gaps in the quantitative laws of simple

conditioning. As a consequence we sometimes are forced to make guesses as to how a particular variable functions or how it interacts with some other variable to determine behavior. As we shall see, our knowledge is particularly deficient in the area of inhibition. We know very little concerning how other variables influence response decrement and we are especially in the dark concerning the effects of nonreinforcement of one response while another response is concurrently being reinforced.

Fig. 4 presents the main portion of the theoretical network formulated on the basis of the experimental variables that we have just been discussing. In order not to complicate the picture too much I have given the schema appropriate for instrumental reward conditioning involving an appetitional need. In the case of classical aversive conditioning it should be understood that the intervening variable D is assumed to be a function of the intensity of the noxious unconditional stimulus and not T_d as indicated, while H is postulated as a function of the number of trials on which the CS and UCS are paired. Shown at the top of the diagram are the experimental variables in terms of which the theoretical concepts or intervening variables are specified. Inside the rectangle are represented the intervening variables. For our present purposes it will not be necessary to specify the nature of the functions (indicated by the arrows) which introduce the intervening variables. Thus it will be sufficient to specify H as being some positive, monotonic function of the number of trials (N_R) that the S-R event occurs. Likewise, D, K, and I_n may also be specified as positive, monotonic functions of their respective determining variables, at least within the range of values of these variables that will be considered.

The interrelations assumed to hold among these theoretical concepts have been based upon the laws discovered in the conditioning experiments we have just been discussing. Thus the excitatory potential (E) of a stimulus complex to elicit a response is assumed to be a multiplicative function of the learning factor H and the drive and incentive motivational factors, D and K. The latter, in turn, are

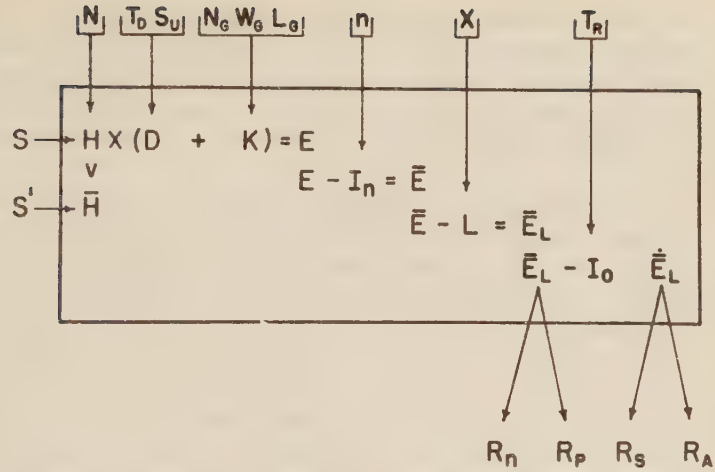

Fig. 4. Diagram of theoretical network showing the relation between the experimentally manipulable variables, the hypothetical intervening variables (in the rectangle) and the dependent response measures in conditioning.

assumed to add. This particular assumption is, to say the least, a most tentative one, for there is no definitive empirical evidence to guide one. However, some exploratory studies involving T_d and W_g have provided data more or less in line with this assumption. In this connection it should be noted that Hull (10) assumed that these two intervening variables (D and K) combined in a multiplicative fashion. It is possible, of course, that neither of these assumptions is correct and that the law of summation may be a negatively accelerated one.

The effects of nonreinforcement of a response in a series of extinction trials is represented in the theoretical schema by the intervening variable, inhibitory potential (I_n), which again may be specified for our present purposes merely as being a positive monotonic function of the number of nonreinforced trials. This assumption is based on the finding that the strength of a response in conditioning experiments decreases with successive nonreinforcements. While the basis of this inhibition is assumed to be competing, frustration-aroused responses

that are established during extinction, the molar intervening variable, I_n, has been introduced to represent their quantitative effects. Subtraction of I_n from excitatory potential E defines the intervening variable effective excitatory potential ($\bar{E}$).

The remaining intervening variables will, for the present, merely be identified with a minimum of elaboration. Later, in connection with selective learning, further details as to the assumptions made concerning them will be treated as needed. The concept L refers to the threshold value of excitatory potential necessary for a response to occur. I_o has reference to the concept of oscillatory inhibition, a notion that Hull introduced to take account of the fact that the behavior of organisms is highly variable from trial to trial. I_o is assumed to vary from one occasion or trial to another according to the normal probability function. These two intervening variables, in turn, permit the definition, in the manner indicated, of two further concepts, superthreshold effective excitatory potential ($\bar{E}_L$). Returning to the left side of the diagram, attention is directed to the concept designated as $\bar{H}$. This symbol represents the habit strength that generalizes to a stimulus (S') that differs along one or more dimensions from the original conditioned stimulus (S). The experimental evidence indicates that such generalized $\bar{H}$ values are less than H, the decrement being a function of the magnitude of the difference between the original stimulus and the generalized stimulus.

Finally, shown at the lower right of the diagram are four measures of response strength commonly used in simple conditioning experiments, response probability (R_p), response amplitude (R_A), running speed (R_S), and resistance to extinction (R_n). Each of these response measures is related to one or other of the intervening variables. The particular functions assumed, linear in the case of R_S, R_A, and R_n and the normal integral function in the case of R_p, were chosen so as to be able to derive the known empirical functions for the several measures.

The first test of this theoretical schema that we shall consider involves a further implication with regard to instrumental conditioning. As we have seen, the theory, as formulated, derives the established fact that level of performance in instrumental reward conditioning is a positive function of the deprivation period (T_d). The further prediction may be made that generalization curves of performance for groups trained to respond to the same stimulus under different drive levels will converge. The derivation of this implication is as follows:

$$ES = H \times D_S$$

$$E_W + H \times D_W$$

$$E_S - E_W = H(D_S - D_W)$$

in which E_S represents the response potential of a strong-drive group and E_W that for a weak-drive group. As may be seen from the third equation, the magnitude of E_S-E_W, the difference between the excitatory potentials of the two groups, is a multiplicative function of H and D_S-D_W, the difference between the drive levels of the two groups. Since habit strength decreases as a function of the distance of the generalized stimulus from the original training stimulus, it may be predicted that the difference between the excitatory strengths of the two drive level groups will decrease as the distance along the generalization dimension increases. Or, in other words, generalization curves for performance measures linearly related to E will exhibit convergence.

Corroboration of this implication of the theory has recently been provided by an experiment of Newman (14) on which rats were first trained to run towards a circle of a certain size under 23 1/2 hours' food deprivation. Following this training, half of the subjects were shifted to a higher drive level based on 48 hours' deprivation and half were shifted to a lower drive (12 hours). The subjects were then further subdivided into four groups at each drive level and given generalization test trials to circles of

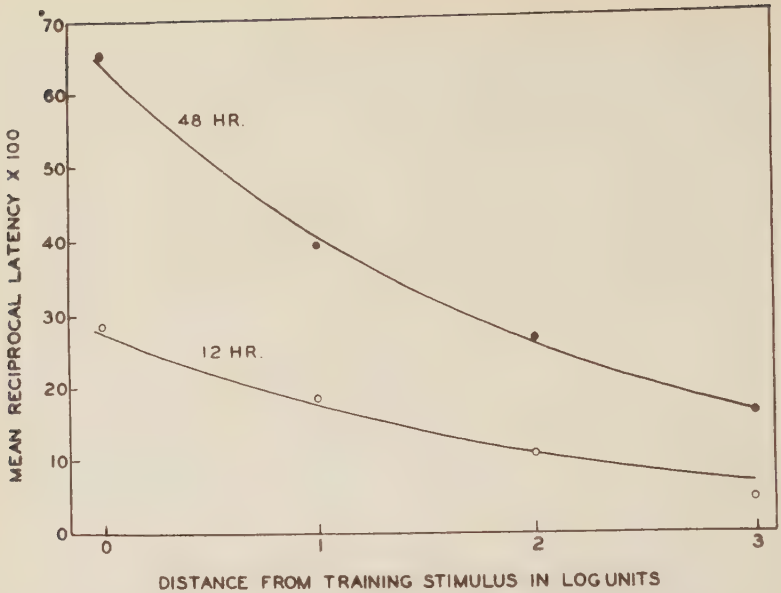

Fig. 5. Data from Newman (14) showing generalization gradients under two different drive (hunger) levels.

different areas. The measures of response strength used were running speed on the first trial and the number of trials to extinguish the response. Fig. 5 shows the level of response in terms of the running speed measure to the original stimulus (0 on the abscissa) and to the three generalized stimuli under the two drive levels. As the theory would lead us to expect, the two curves do tend to converge although the interaction was not significant statistically. The same finding was obtained in the case of the resistance to extinction measure and in this instance the interaction term was significant at between the five and ten per cent levels.

The second example of the testing of our theory within the realm of conditioning phenomena has to do with the effect of varying drive level on differential aversive conditioning. Thus, in the case of the conditioned eyelid response in human subjects, it may be predicted that the degree of differentiation between the positive and negative

CS will be a function of the intensity of the UCS. The derivation of this implication is as follows:

$$E_+ = H \times D - \bar{I}_n$$

$$E_- = \bar{H} \times D - I_n$$

$$E_+ - E_- = D(H - \bar{H}) + (I_n - \bar{I}_n)$$

Here H represents the habit strength of the positive stimulus and $\bar{H}$ the strength of the habit generalized to the negative stimulus. In this instance the drive level, D, is determined by the intensity of the UCS, while inhibitory potential, $\bar{I}_n$, is assumed by the theory to develop to the negative stimulus as a consequence of nonreinforcement. Inhibition generalized to the positive stimulus is represented by $\bar{I}_n$. Examination of the third equation will reveal the fact that the difference between the effective excitatory potentials of the positive and negative stimuli (i.e., $\bar{E}_+ - \bar{E}_-$) is a function of the level of D and hence of the intensity of the UCS. That is, our theory leads us to expect that subjects conditioned with a strong UCS will show greater differentiation between the positive and negative stimuli than subjects conditioned with a weak UCS.

Evidence relevant to this derivation is presented in Fig. 6. This graph pictures the level of performance of two groups of subjects trained with different puff intensities in terms of the percentage of CR's to the two discriminanda. As in the case of the previous data based on this frequency measure we have included only subjects from the middle 40 per cent of the two groups, thus avoiding the ceiling effects of this measure discussed earlier. That the findings of this experiment support the theory is readily apparent. Thus the difference between the curves for the positive and negative stimuli were much larger in the case of the strong-puff group (solid curves) than for the weak-drive group (broken curves). This same conclusion holds in the case of the transformed E measures. The curves for the frequency measure were presented here because of their greater familiarity to most persons.

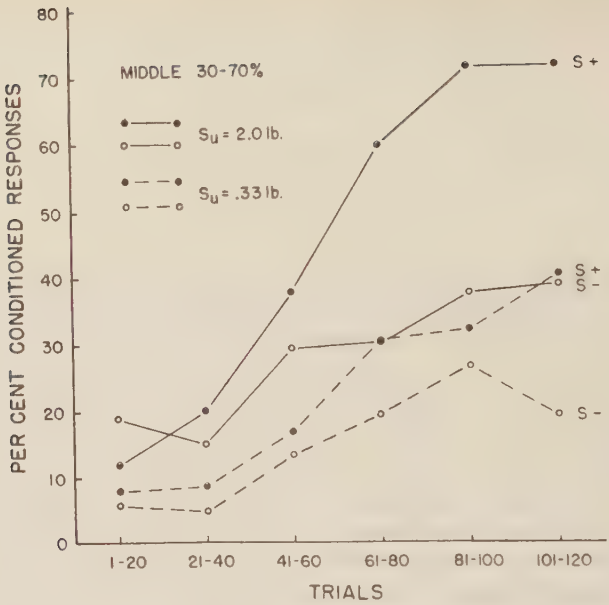

Fig. 6. Percentage of CR's to the positive and negative stimuli for groups conditioned to a strong and weak UCS.

We turn next to the application of our theory to the more complex types of learning tasks that involve selective behavior. In such situations there is, typically, more than one response possibility with the result that selection occurs among the alternatives. One of the most familiar examples of this type of learning task is the single choice-point T maze in which the subject is confronted with two alternative spatial routes, response to one of which is reinforced whereas response to the other is not. Another somewhat more complex kind of selective learning is the type known traditionally as discrimination learning. This type of situation differs from spatial learning in that the subject must learn to discriminate between nonspatial properties of the environment such as different brightnesses forms, or sizes. Best known examples of experimental arrangements involving this kind of learning problem are the Yerkes-Watson box, the Lashley jumping apparatus,

and various modifications of them.

Before we examine the data of these experimental situations in relation to our theory, however, there is one important methodological matter that must be discussed. It has to do with the role in any comprehensive theory of what my colleague, Professor Gustav Bergmann, has described as composition laws or composition rules (2). Bergmann writes: "The fundamental laws, principles, or axioms of a theory are always several, never one. Some of them describe so-called elementary situations, in which only a very limited number of variables interact. At least one is a composition rule, stating how to obtain the laws of complex situations in which many variables interact from those of the elementary situations into which any complex situation can be conceptually decomposed" (2, p. 445).

Applied to our present problem, the implication of these remarks is that a comprehensive behavior theory aimed at encompassing complex selective behavior will consist not only of the elementary systems of empirically based postulates concerning single conditioned responses that we have already considered, but will involve, in addition, some kind of composition law or laws. As Bergmann points out, it is the latter portion of a theory, if it is adequate, that allows for the derivation of a large number of laws for many different kinds of complex situations.

In my Silliman Lectures (20) I made use of a composition law that Hull had suggested for such selective learning. It consisted in analyzing the behavior situation as consisting of two incompatible stimulus-response tendencies conceived to be in competition with each other. Which response will occur on the occasion of each trial is assumed to depend on which at the moment has the greater effective excitatory potential. By means of this composition law and the laws or postulates concerning the variables in the elementary response systems, particularly those relating to oscillatory inhibition (I_O), it is possible to derive a relation between the per cent

occurrence of the stronger of the two responses (R_p) and the magnitude of the difference between their excitatory potentials. This relation which, as we shall see later, holds only under certain conditions, is given by the following equation:

$$R\% = 100 \int_{-X}^{\infty} \frac{1}{\sqrt{2\pi}} e^{\frac{-x^2}{2}} d(X)$$

in which x is equal to the difference between the two competing reaction potentials divided by the sigma of I_o times the 2, (i.e., $E_+ - E_- / \sigma I_o \times \sqrt{2}$).

Before leaving this discussion of the Hullian competing response type of composition law, I should like to make one further comment. It is that I am not too happy with it. In my opinion it is much too oversimplified and while it has worked fairly well in the case of per cent choice measures, it is not adequate to deal with VTE phenomena and the experimental variables that have been shown to affect this kind of behavior. Recently we have been workin with an amplified composition rule which we believe will turn out to be much better than Hull's. Since time does not permit a presentation of the rather elaborate mathematics involved in it and because its implications as far as the per cent choice behavior for the situations we shall be considering are pretty much the same as in the case of the more familiar Hull version, I shall employ the latte Actually, as you may have surmised, this presentation is aimed as much at highlighting the methodological problems that arise in such theorizing as it is at evaluating the adequacy of the particular theory.

Returning now to selective learning experiments, it should be realized that studies of this kind of learning have employed a number of different methods of providing differential reinforcement of the alternative S-R chains. Undoubtedly the most frequently used procedure has been the standard one of reinforcing one of the alternative re-

sponse chains (the so-called positive response) and not the other (the negative response). Closely similar to this in that it also involves the development of inhibition in the case of the negative response is the procedure in which the positive response is reinforced immediately and the negative one only after a period of delay. Other methods have involved differential magnitudes of reinforcement, differential lengths of the alternation response chains, and, with the aid of forcing on some of the trials, differential frequency of reinforcement of the two responses. As has been brought out in previous discussions, the implications of our theoretical network with respect to the effects of drive variation in these several selective learning situations differ greatly. Indeed, in some instances different effects can be predicted for the same selective learning situation, depending upon what the initial or boundary conditions are.

Since the limitations of time will permit only a very small sampling of these theoretical implications and the experimental data relevant to them, I have chosen for discussion here the more or less standard type of situation in which one of the alternative responses is reinforced and the other is not. One of the objectives of our experiments with this type of situation has been to evaluate two alternative postulates within the theory that are concerned with the role of reinforcement (e.g., by food) on the habit strength of conditioned instrumental responses involving appetitional needs. If you will recall, in our theoretical network the habit strength (H) of such an instrumental response is assumed to be a function of the number of times a response occurs in the situation (N_R) and not the number of times it is reinforced (N_G). In contrast to this nonreinforcement or, if you wish, contiguity assumption concerning the learning factor, H, Hull's reinforcement theory assumed that the habit strength of the instrumental response is contingent on the occurrence of a reinforcer.

Now, providing one equates by the use of forced trials the number of times the two responses occur, these two postulates will have quite different implications concerning

the effect of variations of motivational level as the following derivations show:

A. $H = fN_G$

$$E_+ = H_+ \times (D + K)$$

$$E_- = H_- \times D - I_n$$

$$E_+ - E_- = D(H_+ - H_-) + H_+K + I_n$$

B. $H = fN_R$

$$E_+ = H \times (D + K)$$

$$E_- = H \times D - I_n$$

$$E_+ - E_- = HK + I_n$$

Thus, according to the reinforcement assumption that $H = fN_G$, the magnitude of the differences between the competing reaction potentials ($E_+ - E_-$) is a positive function of the drive level, D. That is, the higher the motivational level the greater will be the difference at any stage of training between the excitatory potentials of the two responses. On the other hand, in the case of the assumption that H is a function of N_R, since the habit strengths of the two responses will be equal the magnitude of the difference between the competing reaction potentials will be independent of D.

But these derivations, it should be noted, refer to the effects of motivation level on the magnitudes of the difference between the competing E's. In order to make predictions concerning choice performance, it is necessary to make use of the normal integral function described earlier as relating R_p to the difference between the competing potentials. However, as was hinted earlier and as has been explained elsewhere (20, p. 203), this determination of the per cent choice of the stronger of the two responses holds only as long as the distribution of mo-

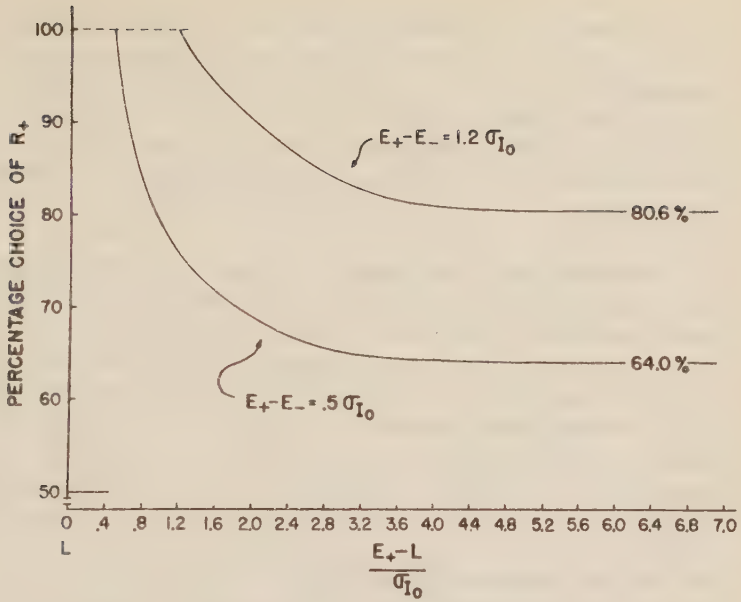

Fig. 7. Theoretical per cent choice of the positive response (R_+) in selective learning as a function of the amount E_+ is above the threshold, L, and the amount of difference between E_+ and E-.

mentary E's for the stronger response is, for all practical purposes, above the threshold L. When both distributions dip below the threshold, the per cent choice for a given difference between the competing E's will be larger the greater the extent to which they fall below L, i.e., the lower their absolute strengths.

Fig. 7 shows how the per cent choice of the stronger, reinforced response (shown on the ordinate) will vary theoretically with the absolute level that E_+ is above the threshold L. The lower curve represents the per cent choice when the differential reaction potential equals $.5\sigma I_O$ and the upper curve that when $E_+ - E_-$ equals $1.2\sigma I_O$. As may be seen from this graph the per cent choice of the dominant response is not a simple function of the size of the difference between the competing E's, but also depends, in the manner indicated, on their absolute

level above the threshold. Because of this it is not possible to predict from the reinforcement assumption that performance in this type of selective learning task will increase with level of drive, even though, as we have seen, it does lead to the implication that the difference between the competing E's increases as a function of the motivational level.

Without going into detail, a family of theoretical curves representing the per cent choice of the positive response as a function of the level of drive, with number of trials as a parameter, can be determined. These curves may be described, in general, as having a distorted u-shaped appearance. Or, in other words, at very low levels of drive and training a relatively weak-drive group would be expected to perform at a higher level than a somewhat stronger-drive group. On the other hand at higher levels of training and drive the reverse picture would hold; that is, the performance of the higher-drive group should be above that of the relatively weaker-drive group.

The implications of the contiguity postulate present a somewhat simpler picture. A similarly determined family of theoretical curves of per cent choice of the positive response as a function of drive, with training again as the parameter, would show no increase with increase in drive. Or, in other words, at no level of training or drive would it be predicted that performance would be higher for a relatively strong-drive group as compared with a group run under a weaker drive. On the other hand, under very low-drive levels and early in training a differential in favor of lower-drive groups would be predicted

From this discussion it is apparent that a rather extensive program of experimental work is needed before one would be in a position to decide between these two alternative theories. Whether or not the research is conducted for the purpose of testing these particular theories is unimportant. We psychologists are going to have to work out many such empirical laws and I, for one, find that it is a lot more fun to do so when one or more theo-

retical assumptions are at issue.

In the remaining portion of this paper I shall describe briefly the findings of some recent experiments of ours that have been concerned with the effects of drive variation on selective learning. Following this a brief summary of the relevant evidence already in the literature will be presented. The first experiment was conducted by Carlton (5) as part of a doctoral investigation. The learning situation used was of the spatial type, one of the alternatives being reinforced and the other not. No correction of the nonreinforced response was allowed. The apparatus consisted of an elevated starting platform and two metal stimulus platforms, one angled to the left and one angled to the right. On choice trials the subject was required to respond by stepping from the starting platform on to either one of the stimulus platforms, the front edges of which were one inch lower and two inches distant from the starting platform. The stimulus platforms could be presented together or singly on either side. The latter permitted forced trials with only one of the discriminanda present on such occasions.

Equalization of the number of occurrences of the two alternative responses was accomplished by administering the trials in blocks of four, the first two trials being choice trials in which both platforms were present and the next two being forced by having only one platform available. The forced trials were arranged so that in any block of trials there was an equal number of responses (i.e., two) to each of the stimulus platforms.

Shown in Fig. 8 are the performance curves in terms of per cent choice of the reinforced platform of two groups of subjects numbering 25 each. As may be seen, the performances of the differently motivated groups (three and 22 hours' food deprivation) obviously did not differ greatly. Nevertheless, as shown by the fact that the 22-hour deprived subjects responded significantly faster than the three-hour group to the positive platform, the maintenance schedules did have a differential effect on the speed of

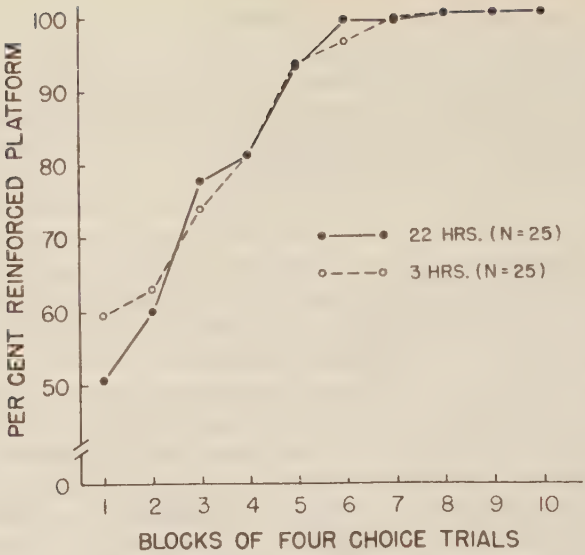

Fig. 8. Data from Carlton (5) showing per cent choice of positive stimulus platform plotted in blocks of four choice trials as a function of drive (hunger) level.

performance. The choice data, it will be seen, are in line with our contiguity assumption. Evaluation of the reinforcement hypothesis is not so simple, however, since we do not know whether we are or are not in the range of absolute drive levels in which a stronger-drive group would be expected to perform better than a relatively weak-drive group.

The superiority of the low-drive group on the initial point is of some interest, for, as you will recall, the theoretical schema, with either assumption, predicts such a result would be expected in the initial stages of training, i.e., low H, and when the absolute level of drive is quite low. In the present instance this finding can at most be described as intriguing since the difference on the initial experimental point was not significant.

Making use of a nonspatial, black-white discrimination situation, we have recently obtained further data

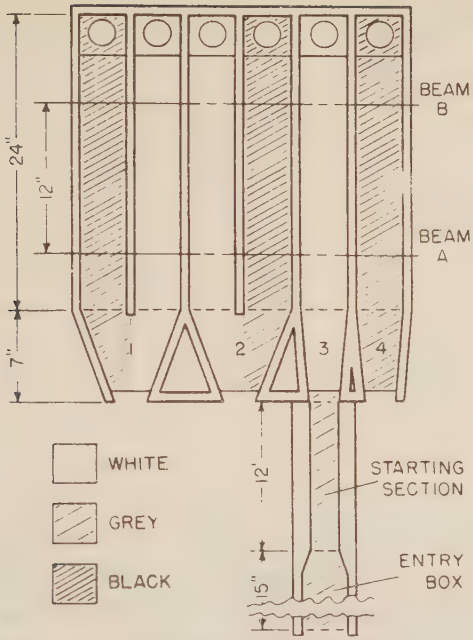

Fig. 9. Ground plan of visual discrimination apparatus.

comparing the performances of high- and low-drive groups (23). A ground plan of the apparatus employed is shown in Fig. 9. A section of the apparatus containing the alleys (shown at top of graph) moved on tracks and permitted any one of four different alley situations to be utilized. Two of these presented the subject with a choice of a white or black alley and two were single, straight alleys, painted either white or black. This arrangement permitted the study of selective learning and differential instrumental conditioning concurrently. Thus trials were given in blocks of four, the first trial always involving a selective learning task with a choice between a white or black alley, while the next three trials involved either a single white or a single black alley. Measurements of speed of starting and running speed on the second and fourth trials of each block provided the differential conditioning data. Only the data from these two trials (second and fourth) was employed because it was possible to counterbalance the order

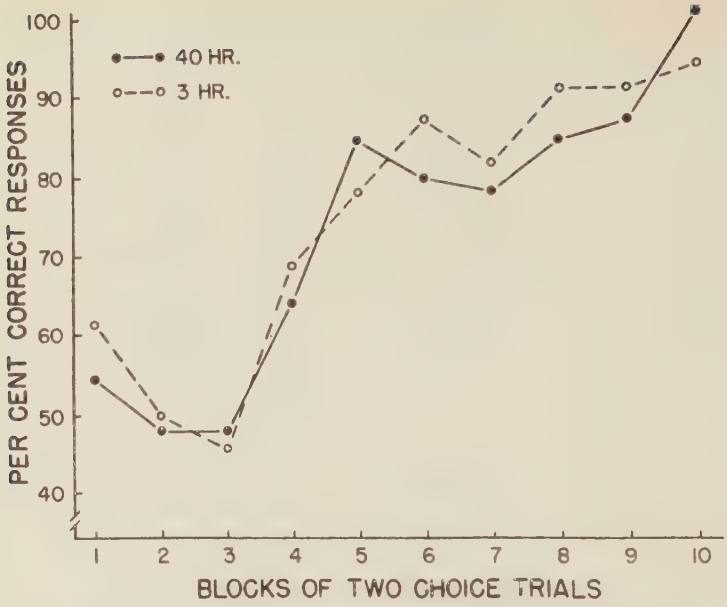

Fig. 10. Acquisition curves of discrimination learning showing per cent correct responses as a function of the drive (hunger) level, with number of responses to the two discriminanda equated.

of the two discriminanda on them. Thus on trial two of each block half of the subjects had the white alley while half ran to the black. On trial four the subject had the opposite discriminanda to that which it had run on trial two.

Response to the white alley was always reinforced while entrance into a black alley was not. No correction was permitted on the choice trials. As in the previous experiment the number of responses to the black and white discriminanda was equated within each block of four trials. One group of subjects was run after 40 hours of food deprivation and the other after three hours. The use of a 40-hour period required that the groups be run every other day.

The results on the choice trials are presented in Fig. 10. It will be seen that they are in agreement with the findings of the previous study using spatial discriminanda. As our

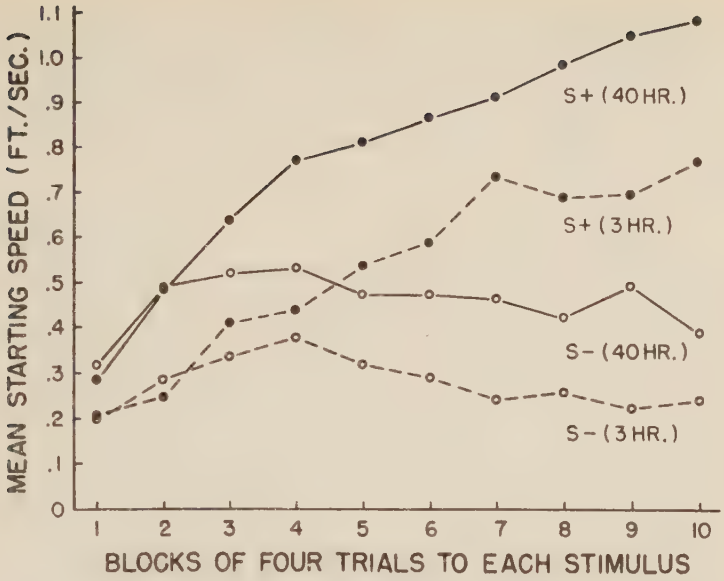

Fig. 11. Curves of response speeds to the positive and negative discriminanda during acquisition of discrimination problem under two drive (hunger) levels, with number of responses to the discriminanda equated.

nonreinforcement hypothesis would lead us to expect, there was no difference in the choice performance of the three- and 40-hour groups. The starting speed data obtained on the forced (i.e., instrumental conditioning) trials are shown in Fig. 11. It will be seen that the 40-hour drive group responded faster than did the three-hour drive group both to the positive and negative discriminanda. These differences were highly significant over the trial blocks analyzed statistically (17-20). If now we assume, as we have, that speed of response is directly and linearly related to excitatory potential, then the <u>difference</u> between the mean response speeds to the positive and negative stimuli directly reflects the difference between the mean positive and negative excitatory potentials, i.e., $E_+ - E_-$. From an examination of the curves it may be seen that these differences appear to be about the same for both drive groups. Indeed, statistical analysis of these difference values for the high- and low-drive groups over the whole training period re-

vealed them not to be significantly different either in the case of the starting speed or the running speed measure. This finding from the differential conditioning data that the $E_+ - E_-$ values were not different for the two drive levels clearly supports the contiguity hypothesis and is not favorable to the reinforcement assumption.

Returning now to our theory, it will be recalled that an essential condition underlying the predictions of the contiguity version with regard to the effects of drive variation on selective learning and differential conditioning was that the number of responses to the two stimulus alleys must be equated. If these responses are not kept equal, the habit strengths of the two responses would be different and the contiguity, like the reinforcement assumption, would predict that a relatively strong-drive group would perform better in selective learning than a weak-drive group, at least at higher ranges of drive level. Similarly, it would predict that the difference between the response speeds to the two discriminanda under this condition would be a positive function of drive level.

In our next study, then, we were interested in ascertaining whether or not three- and 40-hour hunger groups would perform differently in a discrimination situation in which the number of trials to the discriminanda were not equalized. If a difference was obtained under this drive differential when the trials were not equalized, whereas none was when they were equal, the reinforcement assumption would be hard put to account for the findings. On the other hand such a result would be explainable by the contiguity assumption.

While the standard procedure of permitting free choice on all trials would have led to different frequencies of occurrence of the two responses and hence to different habit strengths, we wished to introduce and control differential frequency from the beginning of training. Moreover we were also desirous of studying differential instrumental conditioning concurrently with selective learning. Accordingly, making use of the same apparatus as in the

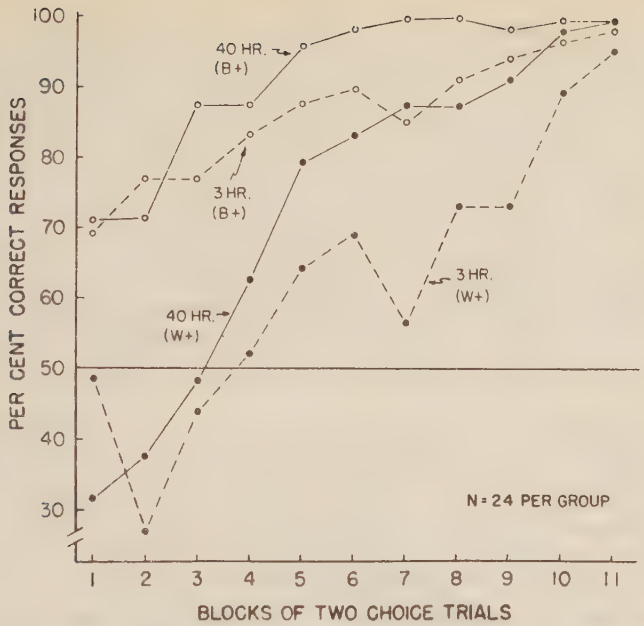

Fig. 12. Acquisition curves on two discrimination problems (B_+ and W_+) under two drive (hunger) levels when responses to the competing discriminanda were not equated.

previous experiment, trials were administered in blocks of three, with two of the three trials being to the positive, reinforced alley and one to the negative alley. The first trial of each block involved a choice between both discriminanda; the next two were forced to one of the single alleys.

Unfortunately the illumination in the experimental room was altered after the first experiment and under the new condition it was found that the subjects exhibited a marked initial preference for the darker alley. Accordingly two further experiments, both employing three- and 40-hour food-deprived groups, were run. In one the white alley was positive while in the other response to the black alley was reinforced. Fig. 12 presents the performance data on the choice trials in terms of per cent

correct responses for blocks of two choice trials after the first. The lower pair of curves represent the data for the subjects trained positively to the white alley, while the upper pair of curves are the data obtained with the groups that had the black alley reinforced. As may be seen, in both experiments the performance curve of the strong (40-hour) drive group (solid curve) is above that of the weak (three-hour) group (broken curve). In terms of total number of errors made in the choice trials, the drive effect was significant at the .02 level in the case of the subjects trained to the white alley and at the .05 level for those trained to the black alley.

Considered together, then, the choice data of these three investigations of the effect of different drive levels on discrimination learning would appear to favor the implications of the contiguity version of our theory. So also, it will be recalled, did the speed measures from the differential conditioning trials of the first of our experiments. Unfortunately, the findings from the speed data on the differential conditioning trials in the last two studies are not only embarrassing to both versions of our theory, but, in some respects, are quite at odds with the findings of the first experiment.

Before examining these new speed data, however, attention should first be directed to the fact, as revealed in Fig. 11, that in our first investigation the 40-hour group responded significantly faster than the weak three-hour group to both the positive and negative alleys. It also should be noted that the strength of the response to the negative stimulus actually increased as fast as that to the positive stimulus for at least eight trials (two experimental points) and continued to increase for some 16 further trials, after which it decreased slowly but steadily. This initial increase with its implication of no inhibitory effect has been found in a number of differential conditioning studies conducted in our laboratory and has greatly influenced our theorizing concerning the nature of inhibition in learning based on appetitional needs. Essentially this theory is an interference one which assumes that the in-

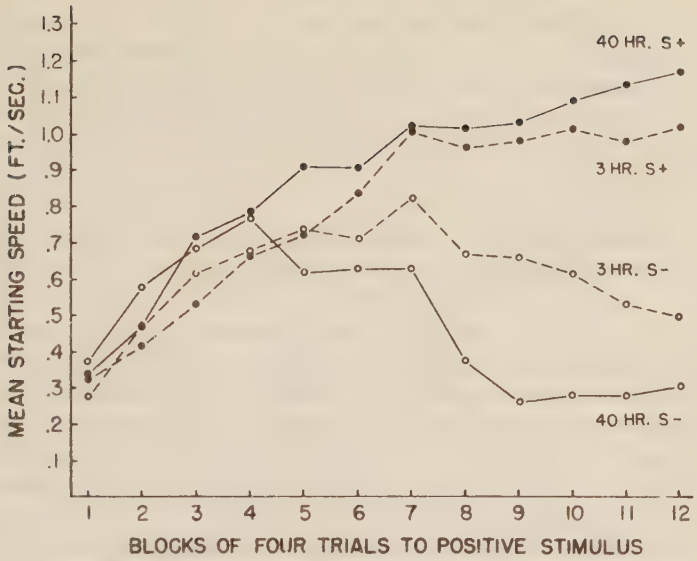

Fig. 13. Curves of response speed to the positive and negative discriminanda under two drive (hunger) levels with number of responses to two discriminanda not equated.

terfering responses are cued to internal stimuli (s_f) aroused by frustration reactions (r_f) to the failure of reinforcement. Such frustration responses, we believe, moreover, do not develop until the animal has first learned to expect food, or, to put it another way, has acquired a conditioned fractional anticipatory goal reaction (r_g). Now the theoretical possibility exists that this frustration effect will be a function of the drive (fT_d) and the strength of the expectation (fN_G). Our first experiment did not support this assumption so far as drive is concerned, for as may be seen from Fig. 10, the decremental effect of nonreinforcement seemed to be about equal for both drive groups. Certainly the curves for the negative alley did not decrease differentially under the two drive conditions. Hence we did not assume that the development of inhibition (I_n) was a function of drive strength (T_d).

As may be seen from Fig. 13, however, which presents

the starting speed curves for the positive and negative responses in our third experiment (B_+ vs. W_-), a quite different finding appeared. Here it will be noted that the decremental effect of nonreinforcement was much greater in the high-drive group than in the low-drive group. Also in disagreement with the first experiment, the response speeds of the two drive groups to the positive alleys did not differ greatly. Indeed, they are quite close together over the last half of the training period. [1]

As a consequence of these conflicting findings we are turning our attention to the study of inhibition in such differential conditioning studies. This has been a much neglected area of investigation and there is, as was indicated earlier, considerable need for further information on the variables that affect this process in differential conditioning. Should future conditioning studies support the conception that inhibition is a function of the antecedent conditions determining drive strength, we shall have to modify our basic theoretical schema. This is, of course, typical of the kind of interaction that takes place between theory and experimental fact in science.

In concluding I would like to discuss very briefly what little other evidence there is from previous investigations bearing on this theoretical problem. Two studies employin the spatial problem (T maze) with equated responses, one by Teel (24) and the other by Teel and Webb (25), obtained results similar to those we have reported. That is, both studies found that drive differences did not affect performance. A doctoral thesis in our laboratory by Loess (12) in which he used a spatial learning task with delayed reward on one side also did not find a significant difference between three- and 22-hour groups. The theoretical

[1] The findings of the second experiment for this starting speed measure were the same as those of the third experiment shown in Fig. 12. Attention should also be called to the fact that the running speed measure gave essentially the same picture in each experiment as did the starting speed measure.

expectations with respect to this situation may be shown to be the same as in the case we have been considering. Including the studies reported here then, we find a total of five experiments in all which have failed to find a difference in such an equated response selective learning situation.

Partly in conflict with these findings, however, are the results of a recent study by Smith and Duffy (19). Of two experiments they report which used equated trials in a T maze, one showed no difference between hungry and food-satiated groups while one did show a significant difference. These studies are somewhat complicated, however, by the fact that the animals were first trained while thirsty to drink in the maze. The experimental training was then carried on with a 20 per cent sugar solution on the positive side and with water on the other. The subjects given four cc. of this solution on the correct alley showed a drive effect, while subjects given only a drop showed no difference. Presumably the animals dran in both alleys if only to find out which contained the sugar. I suspect that the incentive motivation factor is somehow confounded in this experiment with the drive factor.

Turning to the nonequated situations, most of the studies have been of the uncontrolled variety in which free choice trials were used. The results have been, to say the least, highly conflicting. Reporting a differential effect of drive in the simple T maze are the studies of Tolman and Gleitman (26), and Buchwald and Yamaguchi (3). Two studies (5, 6) from our own laboratory, however, failed to find a difference. In the case of the discrimination problem, three investigations (8, 9, 16) have reported a total of four experiments in which there was a significant drive effect. On the other hand a brightness discrimination study by Myers (13) in our laboratory failed to obtain a difference. Thus it would appear that everyone else but us seems to be able to find evidence in line with the theoretical prediction (either version of the theor for this nonequated situation. One possible explanation of our failure is suggested by the fact that an examination

of the running speeds of the subjects failed to reveal significant differences between high- and low-drive groups. This suggests the possibility that the maintenance schedules employed did not produce adequate differences in the hunger states.

Finally, mention should be made of the study by Ramond (18) in which he gave a differential number of reinforcements to the two responses in the spatial situation. Since no nonreinforcements are involved in this situation, one does not have the factor of inhibition to consider. As the theory (either version) would predict, the high-drive groups performed significantly better than the low-drive groups. Taking all the evidence into consideration, then, it may be seen that the batting average, particularly of the theoretical version employing the contiguity assumption, is not too bad.

In this connection I should like to make a final comment on a serious misapprehension that appears to exist among some psychologists concerning theories and their relation to the facts. In the case of more comprehensive theories, at least, it is not a simple question of a theory's being true or false. So far as their relation to the facts is concerned such theories may at any time differ in the number and variety of different phenomena they may encompass and the number of particular experimental findings that would appear to contradict them. One should not lose sight of the fact, however, that a particular experimental result that is in conflict with such a theory is not completely above suspicion. Certainly, if the theory has successfully explained a large and varied set of phenomena and now comes up against one particular negative finding, it behooves the theorist to be quite sure that there is nothing wrong with the experiment before he abandons the theory. This is especially the case in the field of psychology which is, unfortunately, plagued by too many purely negativistic critics. From my own experience with such persons I have become greatly impressed not only with the reality of the concept of the "unconscious" but also with its ingenuity. The extent to which such critical experimental

attacks manage, either to neglect to take into consideration the initial and boundary conditions specified by the theory, or fail to recognize the role of some counteracting factor, is truly amazing. Apparently these critics do not realize that theories of the more comprehensive type do not get eliminated by such means. Even a theory that works only 90 per cent of the time is better than nothing at all. The history of the careers of scientific theories in other fields shows that they disappear or are abandoned only when some better theory comes along.

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COMMENTS ON PROFESSOR SPENCE'S PAPER

by
Robert C. Bolles

In the first part of his paper Spence has set himself the difficult task of summarizing much of his book, Behavior Theory and Conditioning. One difficulty is that he has adopted some methodological procedures that appear to be justified in the larger context of the book, but seem a little devious in the smaller context of the present paper. For example, throwing away data from all but the modal third of Ss, transforming to units of variance, the arbitrary scaling of the different response measures, all these procedures seem a bit mysterious, but a rationale for each of them can be found in the book. Their justification is essentially pragmatic: Using these methodological procedures Spence can put into order a diversity of data from classical and instrumental conditioning situations. The success of the theoretical network within this domain can hardly be doubted.

The next question is how far into other domains the theory can be successfully extended. Spence has pushed into discriminative conditioning by introducing the concepts of generalized habit strength and inhibition, $\overline{H}$ and $\overline{I}_n$. In the second half of his paper Spence reports on a number of selective learning studies designed to test his contiguity theory of reinforcement against the Hullian drive-reduction theory.

The response-probability measures in these studies follow a simple pattern, in which drive is not a parameter, but the speed measures are inconsistent. (One wonders why a test was not made in a classical or instrumental conditioning situation where, presumably, the interrelationships among response measures are better understood.) It is true that these results don't seem to follow from Hullian theory, but it is not clear that the drive-reduction postulate is to blame. The trouble might just

as well lie with any of the theoretical links intervening between habit strength and observable behavior.

In this connection Spence has pointed out that his theory, too, fails to account fully for the data. Perhaps, as he suggests, Spence can repair the theory with a new conceptualization of inhibition. On the other hand, perhaps the boundary conditions of the conditioning theory are violated in selective learning situations. Thus, perhaps the linear relationship found in conditioning studies between E and R_s only applies in a one-response, non-discriminative situation; in more complex situations, other, more complex laws may apply.

Even within the framework of the contiguity theory, one feature of Spence's results seems strange to me: Shouldn't the probability of responding to the rewarding alley be raised in the high-drive group because of the action of r_G? I would expect the cues from the rewarding alley to arouse stronger, larger, or more frequent r_G's in the high-drive group than in the low-drive group, thereby leading to a difference between groups.

Finally, a point about the multiplicity of energizers: Classical drive theory assumes just one; Spence's theory involves two, D and K; Estes suggests that we don't need any. While I can't agree with Estes that we can get along without any, I do wonder if we really need to have two. Are both D and K necessary concepts or are they somehow redundant? While it is true that D and K have been defined in the past by manipulating different experimental variables, is there any reason to suppose that K is not a function of T_d? The only possibly contrary evidence I know is that from Spence's visual discrimination studies just discussed. Both Estes and I have argued that the D multiplier is a superfluous concept; Spence and his students have reported a number of studies that indicate the usefulness of an incentive motivation factor, a K multiplier. Would Spence's theory suffer in any way if the D term were just removed, and the whole burden of energizing placed upon the K term?

One possible rationale for maintaining both D and K is to apply D to aversive behavior and K to appetitive behavior. But why not be parsimonious? Aversive and appetitive behavior can be given parallel interpretations by means of the assumption that the motivating agent in aversion (in so far as one is needed) is the fractional anticipatory escape response. An escape response may be defined as one that terminates noxious stimulation. In this view, a danger signal acquires its energizing (motivating) power by virtue of its coming to arouse anticipatory escape responses, just as other situational stimuli may energize appetitive behavior by arousing fractional anticipatory consummatory responses.

COMMENTS ON PROFESSOR SPENCE'S PAPER

by
W. K. Estes

When one is concerned, as both Dr. Spence and I are, with tests of quantitative assumptions, one must be prepared continually to find that the chain of reasoning from theory to data depends critically upon meticulous attention to technical details of mathematical derivation and experimental design. This observation is illustrated by two points that arise in connection with Dr. Spence's paper.

1. While exhibiting several families of empirical curves relating response speed to number of learning trials at different drive levels, Dr. Spence commented that the curves of a family diverge early in learning, as required by both his theory and mine, but do not clearly show the subsequent convergence required by the latter. We recall that predictions from the stimulus theory were couched in terms of response probability. One may appropriately estimate response probability on any trial from response speed, defined as the reciprocal of mean response time. But one should not lose sight of the well-

known mathematical fact that the reciprocal of a mean is not generally equal to a mean reciprocal. Predictions from the stimulus theory concerning response probabilities do not apply to curves plotted in terms of mean reciprocals (of individual response times) as Dr. Spence's evidently were. From published sources on the Hull-Spence theory, it is not clear to me which of the two measures of response speed is supposed to exhibit the multiplicative relation with deprivation time; it is clear, however, that the multiplicative relation cannot hold for both measures.

2. Experiments designed to test theoretical predictions concerning the influence of drive level upon measures of learning or performance ordinarily involve the assumption that other important variables are held constant. But in the case of conditioning experiments, whether of the classical, escape, or avoidance variety, this assumption is probably rarely, if ever, satisfied. When classical conditioning has been studied in relation to the Hull-Spence theory of drive, the level of drive has been defined in terms of intensity of the unconditioned stimulus. Thus any differential reinforcing effect of more vs. less intense unconditioned stimulus, or of greater vs. smaller amplitude of unconditioned response, is confounded with drive level. Similarly in escape or avoidance conditioning, drive level, defined in terms of stimulus intensity, is commonly confounded with the amount of stimulus intensity reduction that follows the reinforced response. It is quite possible that these confoundings can be completely untangled only within the framework of a theory. If so, one must be all the more careful not to test one's theory by means of an experimental design which presupposes that the theory is correct.

Dr. Spence has clarified the structure of his theory by making it clear that $\underline{D}$ is to be regarded simply as a term in an empirical law while $\underline{S}_D$ is a term in a theoretical model. Up to the present the two concepts have evolved rather independently, but the possibility remains that the properties of the "empirical construct" $\underline{D}$ will

prove to be derivable from the model. It should be emphasized, though, that in the Hull-Spence formulation, the multiplicative law which defines D is assumed to hold for all empirical situations to which the theory is applicable. My own view is that the empirical relation between deprivation time or stimulus intensity, on the one hand, and measures of habit strength, on the other, takes on a multiplicity of forms, each applying at most to a limited class of experimental situations. Thus we must expect to find, and I believe do find, a collection of empirical laws each of which could be said to define a different D construct. What we can hope to accomplish by means of a theory is to derive the whole collection of empirical laws from a single conceptual model. Fortunately our chances of success at this venture do not stand on our ability to force all of the empirical laws into a common form. I suspect that in this matter Dr. Spence has inherited an untenable theoretical position and that even his well-known experimental resourcefulness has sufficed to hold it this long only because of the total absence of alternative theories, a tactical situation which cannot be expected to last indefinitely.

Considering all of the Symposium papers, I find one particularly cogent implication for future research. It would seem that at least in the area of elementary learning situations, the age of "high-drive vs. low-drive" research should be nearing an end. Theory and experiment agree that relationships between behavioral measures and experimentally defined drive levels are typically curvilinear. Consequently it is rarely possible to justify any general all-or-none statement specifying whether a high drive, as compared to a low drive, will facilitate, or impair, any given aspect of learning or performance. One of the principal values to be anticipated from the development of quantitative theories is that they may speed the replacement of misleading categorical generalizations with statements of functional relationships. The parametric study needed to establish a functional relationship is likely to seem far more attractive to the potential investigator if the finally plotted curve can be expected

not only to find an obscure niche in the next Handbook but also to play some part in confirming or refuting his own or his neighbor's theory.

MOTIVES, HISTORY AND CAUSES

Richard A. Littman[1]

University of Oregon

Salviati: I shall say that that which makes the earth move is a virtue like that by which Mars and Jupiter are moved. . . .

Simplicius: The cause of this is most manifest, and everyone knows that it is gravity.

Salviati: You are out, Simplicius; you should say that everyone knows that it is called gravity, and I do not question about the name but about the essence [essenza] of the thing. Of this you know not a tittle more than you know the essence of the mover of the stars in gyration, unless it be the name that has been put to the former and made familiar and domestic by the many experiences that we have of it every hour of the day.

-Galileo

There is no word in modern philosophy repeated more often than this one [force], none which is defined so inexactly. Its obscurity makes it so handy, that one finds its usage is not restricted to bodies with which we are familiar; an entire school of philosophy today attributes to beings which have never been seen a force which does not manifest itself in any phenomenon.

-Maupertius

Now Freud's contribution to science is notable, and in my opinion epoch-making, for a reason which has

[1]I wish to express my appreciation especially to Drs. K. J. Lambert and G. Patterson for their disturbing help. Drs. L. Postman, D. R. Miller, and E. Rosen, and Mr. R. Katter have read earlier versions and shaped this final version by the problems and questions they raised.

hardly ever been mentioned. And this reason is that he has given to the science of mind a "causal category"; or, to put it less academically, he has given us a key to the explanation of mind.

-E. B. Holt

All that I have to say could be said in about five minutes. Indeed I am sure I could say it better in that short time than I shall with the longer time I will take. However, if I actually spoke for only five minutes you would feel twice cheated: once, because you expect something more than a five-minute talk and second, because you expect to be confused and even bored. Since I am too compliant a person to want to upset your expectations, I shall follow the usual rules and talk too long, saying too much about too little.

The amount of time and effort that psychologists spend discussing and working with motivational phenomena is incredible. Yet it is evident from previous Nebraska Symposia and other sources that there is still no substantial agreement about what motivation is. I think there is something wrong when something like this persists for as long a time as it has. So I shall talk about why there is no unanimity. I want to discuss why the concept of motivation raises so much heat and why psychologists say such outrageous things about it. In short, I want to talk about why motivation is so important.

The nub of the problem, I believe, lies in the following observation: Psychologists and nonpsychologists invent and use motivational terms in the same situations for the same purposes or reasons. They apply motivational terms to describe and explain the conduct of animals--human or otherwise. Certain aspects or properties of behavior are called motivational in contrast to others which are grouped and labeled by other words. The psychologist (and the nonpsychologist, too) tries to formulate the uniform and essential characteristics or properties of motivational phenomena. He attempts to discover their mode of operation and their causal history and consequences. He espe-

cially tries to relate motivational phenomena and what he knows about them to nonmotivational phenomena so that he can reach his objective of explaining and predicting the conduct of organisms. In order to do this properly he should be able to distinguish between motivational and nonmotivational phenomena explicitly. So far, he has not been able to do this.

Out of the rough agreement of common usage have come distressing differences. It is clear, for example, that for certain people drives are motivational; for others instincts are; for some motives; for still others set, purpose, desire, fear. Each person fashions a definition that is sure to provide for his favorite concept; it usually does not provide for those of many others. When all the definitions are considered, however, it develops that all the concepts are provided for; unfortunately, they are not all incorporated under any one proposed definition. Hence the conflict.

Before I say any more I want to emphasize one thing. When I use the terms "motivation" and "motivational," I am using them in a generic sense. Some people prefer, for example, to use "drive," or any of the other kinds of words I have just listed for you. So when I speak of "motivation" I mean the category of which "motive" is only one kind of instance. "Motivation" will be used as the broadest sort of term--to cover a whole host of things; I believe this is the sense in which it has been most often used in these Symposia. Another word frequently used for the same purpose is "conation," and I shall use it, too; later on, I shall have an interesting tidbit about this latter word, by the way.

In order to concretise the situation I have just been pointing to, let me read two lists of words to you:

List I

1. ability
2. abstraction
3. aphasia
4. apparent movement

5. aptitude
6. belief
7. conditioning
8. closure
9. deafness
10. end spurt
11. forgetting
12. generalization
13. habit
14. imitation
15. intelligence
16. introspection
17. knowledge
18. learning
19. loudness
20. masking
21. mental age
22. perception
23. protanope
24. retention
25. sense threshold
26. thinking
27. time-error
28. Weber fraction

I imagine the list strikes you as pretty miscellaneous.

Now, listen to the words in List II.

List II

1. agitation
2. aim
3. alerting
4. anxiety
5. appetite
6. arousing
7. aspiration
8. attitude
9. aversion
10. central motive state
11. complex
12. compulsion
13. craving
14. decision
15. desire
16. determining tendency
17. disposition
18. drive
19. élan
20. emotion
21. encouragement
22. erg
23. focussing
24. frustration
25. goal-orientation
26. impulse
27. incentive
28. inclination
29. instinct
30. intention
31. interest
32. motive
33. need
34. orientation
35. passion
36. phobia
37. preference
38. proclivity
39. propensity
40. purpose
41. sentiment
42. set
43. stimulus
44. tendency

45. teleology
46. tension
47. tropism
48. urge
49. volition
50. want
51. will
52. wish

The second list is also quite a miscellany, isn't it? But is it not also different in type from the first list? Do not the two lists, taken each as a whole, hang together and contrast with each other? My talk will try to clarify why this is so, why the contents of the two lists each go together and why they are kept separate from each other.

I think we all know what to call the terms in these two lists. The first list consists of nonmotivational terms and the other consists of motivational terms. The ones in the second list show up regularly whenever psychologists deal with motivation; a hasty review of contributions to these Symposia, for example, will provide ample evidence on that score. It is also true that these terms (with their synonyms or instances) are also used by writers of fiction (good and bad), moral philosophers, and that wonderful being--the man in the street--as well as psychologists. Further, as I have indicated above, I believe that these different kinds of persons all use these terms in much the same way. To be more accurate, I should say they use terms of this sort under similar circumstances and for similar reasons and purposes though they may disagree very strongly as to <u>which</u> is the correct term on any given occasion.

Now, I am sure that this is not news to anyone. We all know that if a person has the capacity to acquire the use of language, he will learn that certain words are used to talk about certain sorts of things. The occidental child learns eventually that when a toy train bumps him he shouldn't say it did so "purposely" but that it did so because <u>he</u> "got in its way" (accidentally or purposely, as the case may be). He learns that only living things (perhaps only of a certain sort) can have List II-type words applied to them.

Since psychologists are children of their culture it is not surprising that they also learn these things. But

when we become psychologists (or some similarly trained persons) we are forced to use the language of everyday life to talk about newly discovered phenomena and insights. However this usually involves a certain amount of discomfort, because it entails violating some pretty basic cultural precepts, the controls of language usage. So what I want also to talk about is why there are these violations--I should say, apparent violations--and point out what we can do to mitigate the confusion and distress that such "illegal" activities produce. After all, these are concepts that we have invented and we should at least be able to make them do what we want.

The best way to understand this web of problems is to consider why these words go together, to try to understand why they have been classified together. While it may seem to be an odd way to proceed, I believe it will give us a significant insight into the problems we face in the scientific study of motivation. The reason I am taking this tack is simple, though the exposition is not. First off, it is obvious that I am not talking just about words; I am also talking about the things these words are about, which is what all of us interested in motivation are primarily concerned with.

As I have suggested above, however, it is not only we psychologists who have this interest and need; other scientists--biologists, sociologists, and anthropologists--have them; the student of the humanities--the classicist and the literary critic--also has an investment in motivation; and the man in the street--whether a skilled, perceptive person or an inarticulate, insensitive brute--he too has an interest and need in motivation. Though I would not push this point too far--it would give my literary colleagues too much courage--all of us know people who, though not psychologists, are far more skilled in the use of motivational language than many psychologists, who are able to deal with motivation more discriminatingly and effectively. While many of the outstanding people who have this ability are writers (though far too many writers don't), I believe that the ability also exists in

almost every occupational or social grouping that we might look at--lawyers, bartenders (especially), shoe salesmen, shop stewards, executives, and farmers.

"Well," you might say, "I've heard that claim before, but I don't believe it." Unfortunately, I cannot offer you any convincing evidence now that it is so, though I do believe it is. However, it is not really crucial to my analysis to argue or prove that there really are nonpsychologists who are better at this sort of thing than psychologists. It is only necessary to grant that such people (everyone, that is) play at the same sort of game that psychologists do, even though they may lose more often than not. Because I think you will see that neither the use of, nor the concern with, motivational concepts is a distinctively psychological problem. There are many things for psychologists to do about motivational phenomena, but preemption is not one of them.

To return to the reasons for asking why these words go together: If one considers the various proposals that have been advanced at these Symposia and elsewhere for defining motivation, it is brutally apparent that there is no agreement--from which it might reasonably be deduced that perhaps there is no agreement because there is no basis for it; that the words in the number II list really don't go together after all. However, the fact that so many people agree on what the instances of motivation are urges a less traumatic solution until, at least, some good idea of why the grouping occurs can be found. I shall argue, therefore, that the terms very definitely do belong together though not for any of the reasons so far suggested.

We shall solve our problem if we realize that it is not one of "What are the concepts which are motivational?" We know what they are. The disagreement is about what the defining properties are of concepts that are already classed as motivational. In a quixotic sense, we already know what motivation is. At least we do by the test of what words we apply on what occasions to what behavior;

we just aren't able to agree on why we are able to do it.

To put it another way, we do not have a problem about what to do with new and disturbing concepts or facts, but what to do with old concepts and old facts; not where to put new terms but how to rationalize old terms; how to relate new knowledge and theory to concepts which have already been put in their logical and psychological places. Since there is little question but that psychologists and nonpsychologists put the same concepts in the same place, the fundamental question in understanding motivational controversy is indeed one of asking, "Why are these terms grouped together?" And this is not the same thing as asking, "What is the definition of motivation?"; it is rather to ask, "Why are these called motivational?"

Before matters get too far out of hand, let me tell you now what some of my conclusions are. I do not believe that there is or will be found a significant theoretical, i.e., a fruitful psychological, basis for rationalizing past, present, or future motivational concepts. I will suggest that motivational terms--and hence motivation, which is merely a generic term--are therefore quite heterogeneous. I will argue that in terms of the kinds of things that the contemporary psychologist is interested in there is only a trivial communality among these motivational terms. I will try to show why, even though the concepts do have some communality, we can still regard them as heterogeneous, and I will further argue that psychologists would be better off if they didn't think these concepts had to go together.

Therefore, I will conclude that there are no general answers to such questions as: (1) Where do motives come from? (2) How do motives operate? (3) How can motives be changed? (4) How can motives be manipulated? (5) How can motives be aroused? The answers to such questions must be devised for each type of subclass of motivational phenomena. These subclasses have been grouped together on the basis of historical accidents, and do not represent anything that can profitably be regarded as a

psychological unity. Rather, they represent a collection which history has donated to us. However, as I shall show the collection is not arbitrary and is by no means the resul of silliness; it is merely the result of faulty analysis. But it surely will be silly if we perpetuate this state of affairs. For things are called motivational for the same kind of reason that regiments and divisions are called "military," and this is a different sort of reason from why falling bodi shooting stars, and flying saucers are called "gravitationa

I. The Definitional Approach to Motivation: Some Typical Proposals

Let me outline the strategy of the remainder of the paper. I will first present you with some standard ways of looking at motivation and point out why no one of them is comprehensive enough. Then I will offer a definition of motivation which will incorporate all motivational terminology and concepts. Following that I will indicate why such a "silly" definition is the only one which will do. This will be done through the sketch of some history and the study of a logical problem. Then we will be finished.

Throughout the subsequent discussion I shall assume that the words in the motivation list do go together, and I shall use that assumption in commenting upon the various proposals for defining motivation. So we shall be putting each definition to a test which consists of asking how well it incorporates the various concepts that are conventionall i.e., by some respectable psychologists, considered to be motivational. I must be somewhat tedious about this all so that you can see clearly the scope of the problem confronting us and why an analysis like mine leads to the only kind of sensible long-run solution. When I am finishe I hope it will be clear why my assumption about the list of motivational terms is not arbitrary.

The various approaches to, and aspects of, motivation that I shall talk about under this heading are as follows: energizers, directors and selectors, persistence, how-

to-go-about-defining-motivation, internalism, motivational physiology, and awareness. I say a great deal about the first of these because there is a great deal that needs saying, but also because it enables me to lay out the major mode of attack; the remainder of the topics are then considered in terms of the special or additional problems that arise in connection with them.

A. Energizing Function Type of Definition

Let us turn first to the proposals of Brown in the initial Symposium of this series (4) and Farber in the second (9). Their discussions are among the most thoughtful expositions I know of the idea that motivation refers exclusively to conditions that energize behavior. Brown puts it like this: ". . . drives function in combination with existing reaction tendencies to produce overt behavior," while Farber says "that a given variable has the characteristics of a drive or implies the presence of a drive state if [he describes a reinforcement principle]. . . and/or the presence of the variable energizes or intensifies whatever reaction tendencies exist in the given situation." They assign to habits associated with drive stimuli or other stimuli the functions of directing or selecting behavior.

The analyses of Brown and of Farber are dangerously ambiguous or vague with respect to a number of issues. The most important is the fact that the very notion of "energize" or "activate" is not elaborated. Hence it is impossible to decide among, for example, the following possible interpretations: (1) drives and habits jointly energize behavior; (2) drives energize habits which then energize behavior; (3) drives energize behavior which displays the properties of the habits underlying it; (4) drives energize habits and they jointly energize behavior; (5) (after Farber and Melton [21]) drives lower general or specific reactions thresholds so that (and because this is left unsaid, I apologize for putting words into Farber's mouth) when an appropriate stimulus comes along it will be able to trigger off behavior. On this last interpretation, of course, drives don't even instigate behavior, they just

prime it. I think that each one of these possible interpretations leads to different research and theory. But what is most important to note is that it is not clear from their exposition why it is not also true that, for example, habit energizes behavior. I think I can show why they have been forced to put their main thesis so vaguely; while you will have to wait for the full details, let me say now that the source of the trouble lies in the very notion of "to energize." When I am finished, I believe you will see that it makes equally good sense to say that habits energize drives, etc. Meanwhile, I should like to indicate on still other grounds why their proposal is not satisfactory.

One has only to consider the reactions to their proposal to see where the trouble lies. For an example, let us take Harlow's comments on Brown's paper (14). He argues that there are positive motives as well as negative ones like anxiety or fear (which Brown suggests is the motivational basis for a great amount of learned behavior). He also rejects the notion that drives cannot be directive by referring to the head-turning behavior of infants when they are hungry; this is a directed behavior which is innately linked to the hunger drive. He concludes, "Furthermore, I cannot subscribe to any assumption that pain or conditioned pain (anxiety) is nondirectional or that pure drive and pure habit aspects of such performances are separable" (14).

In his own talk, Harlow goes to considerable trouble to emphasize the role of positive motivation, whether the sources are inside or outside the organism. He even suggests (as have, significantly enough, a number of other contributors to these Symposia [11, 23]) that it seems reasonable to him that the very problems with which organisms contend provide their own motivation, an instructive parallel to the suggestion of Woodworth many years ago (32).

The general import of Harlow's remarks is that people like Brown and Farber are in error because they just have not got hold of all the kinds of drives that there are.

They have selected out those drives which appear to have predominantly energizing properties, i.e., the homeostatic drives, and they have therefore been misled. His argument on this is that, when you get right down to it, "Motivation results from the activation of 'brain centers' by chemical substances or afferent impulses, and it does not matter whether these hormonal effects or afferent impulses are initiated by exteroceptors or interoceptors" (13). He illustrates this point by referring to the externally aroused drives like manipulation, exploration, curiosity, play, and tropisms about which he has given so much knowledge to contemporary psychology.

Now I must say that I believe Harlow misunderstands the Brownian proposal quite thoroughly. But the nature of his comments inadvertently reveal what is wrong with it. The important thing about Harlow's remarks can be seen if we imagine the dispute between Brown and Harlow to be taking place, say, 250 years ago. Is there any reason to believe that the line of their argument would be different from what it is now? I do not think it would be. This shows that what motivation means to Harlow has nothing to do with his knowledge or belief that there either exists a nervous system or that he knows how it works or believes that it works in certain ways. This means that for Harlow there are certain aspects of behavior that he classifies as motivational independently of his explanation of them. And this is also true of Brown and Farber, of course, just as it is true that they all use motivational concepts to explain other aspects of behavior. The fact that for Harlow drives are "vectors," that, to bastardize a Freudian notion, "energizers are bound, not free," is undoubtedly important. It comports with the significant insight of Postman (26) that the inability to distinguish between drives and drive stimuli because all the defining conditions are identical, renders the notion of "pure" energizer logically indefensible. As I shall show later on there is nevertheless a sense in which Brown and Farber and Harlow are quite in agreement, and it is this sense which leads Harlow to say to Brown and Farber, "You are not pointing to all the drives there are" and Brown and Farber to say to most everyone else, "You

are calling too many things drives."

It should be noted, however, that drive is only one out of many motivational concepts. Therefore I shall now do for drive in general as a category what Harlow has done for drives in detail. Both Harlow and Brown agree that drives or motives (they use the terms interchangeably --a practice which offends many psychologists who prefer to use "motive" as the generic noun rather than "drive") must entail, at the very least, the capacity to energize behavior. But consider the following terms which I have selected from the motivation list: "wish," "determining tendency," "preference," "sentiment," "set," "appetite," "orientation."

Surely these are all motivational terms. Yet they differ in a most important respect from drive, as analyzed by Brown and by Harlow (for their analyses are not the only kinds for drive, and that complicates the picture very much). None of these terms refers to energizing or activating properties. They refer rather to the fact that the organism is ready to behave or be activated. This is not by any means the same thing as "having a reaction tendency, as Brown or Farber might be tempted to say--it is to have a reaction tendency that is primed to go off in contrast to the same reaction tendency when it is not primed. Short-run "sets" and "determining tendencies" illustrate this kind of motivational phenomenon. Some of the other motivational terms refer to the tendency or likelihood of becoming ready to be activated, to the fact that there may be exceedingly long-enduring motivational phenomena--so long lived, indeed, that we even use them to characterize persons as, for example, "aggressive," "competitive," "dependent," etc.

In both cases there is no on-going process or condition which must act on, or interact with, habits to produce behavior now, and here. One could postulate a "trace" or "reverberating circuit" to provide a process or condition; but this would not help much because what it would stand for is already known. A person may not behave in certain

ways for years, and still he will have a motive (or drive, if one finds that term more comfortable). Reflection will show that there are distinguishing features for other motivational phenomena which do not at all rely on the presence of an activating or energizing process in the here-and-now as the defining criterion. I assume that what is meant by an "energizer" is some condition of short duration which comes into and goes out of existence in relation to certain types of behavior. I shall show later on another sense it might have, but we shall then see that the Brownian proposal loses its concreteness if that is what is really meant --as I think it does.

Hence the energizing proposal is untenable because it does not permit the incorporation within the area of motivation of the greater majority of concepts which are considered to be motivational. Now I am quite aware that Brown could argue as follows: "Whoever said that I was trying to account for all the terms in your list! I have been making a proposal to the effect that we should restrict the meaning of the term 'motivation' only to those properties of behavior which are energizing. I believe this is a more fruitful approach to behavior. You're just logic-chopping, playing dictionary games." But if this is what Brown is doing, making a proposal that we change to his analysis or give up an older way of thinking, then he has indeed lost the game. People are just not buying his proposition. And they are not buying it because it excludes from motivation what people want to have in it. Nor does he even offer them any increase in power of prediction or explanation as a reward for giving up their conventional classification. He attempts to provide an explication or analysis of the concept of motivation, and he does so, in the main, by throwing out what people want left in; he hasn't analyzed, he has reclassified.

So his recommendations will not be followed. Nor for that matter will anyone else's, as we shall see. Psychologists should not be too upset about it though; it really doesn't make much of a difference, one way or the other, for what Brown (and other psychologists) want most to do

and, in fact, do.

B. Directive and Selective Functions of Motivation

It will not be necessary to go deeply into a consideration of the definition of motives in terms of their directional or selective properties. In general, the terms reveal the ways that psychologists have come to talk about the adaptive aspects of the purposive, goal-oriented or teleological properties of behavior. As such, it represents a venerable sense of motivation.

The arguments advanced for defining motivation in these respects follow much the same line as those for an "energetic" type of definition. Certain aspects of behavior, usually with respect to goals or some other feature of "end-states," which are already taken to be motivational, are characterized in terms of selection or direction or both.

Aside from the various problems associated with specifying what exerting a directive or selective effect or control might be like (and there are many serious questions here) the important thing to note is that there will be some motivational concepts which are disenfranchised. Whether the directive or selective functions are taken singly or jointly, it is clear that "drive" in Brown's sense (as well as that of most physiologists), "impulse," and "urge" are out. Anything which is just an energizer is out.

Nor are matters helped by saying, "Well then, let us include energizers with these two." The definition then becomes even more restrictive, and concepts which are either directive or selective, or even both, but which do not include energizing properties, are knocked out. This would be the fate, for example, of all the tendency and likelihood motivational terms, a class of terms which constitutes a big portion of all motivational terminology.

Now, one might wish to argue here that there really

aren't any such things as pure energizers, or pure directors or pure selectors, that in fact they always go together. To be sure, they do. But then it is also true that these three sorts of things as a sort of syndrome are themselves not really pure because they are always related to some sorts of habit systems, or cognitive structures, and both are, of course, always affectively or emotionally tinged. So we come to the fact that when you get right down to it there is just a total organism behaving in a total way. Quite so! And who would deny it? But the job of the scientist and analyst is to make distinctions about what exists as a totality, because that is the way one gets to understand the totality. Hence the question is one of whether it is possible to discover a property of energizing or directing; if this can be done, then one can speak of energizers or directors, whatever else they may be. The question, therefore, is whether the basis for calling things motivational is or is not in terms of some one or a combination of these properties or notions. However, there is a germ of truth in this contention; I hope to fertilize it and show you when it has grown that anything can be a director or energizer and hence anything can be a motive. But this is different from saying that all motivational concepts include at least the three functions we have so far spoken about.

C. Persistence

It has been suggested by a number of persons, e.g., Peak (25) and Atkinson (2), that the thing which makes us posit motivational concepts is the persistence of behavior. As both Peak and Atkinson conceive of it, persistence is always in some direction and with respect to various means, so the directive and selective properties of motivation are also in the picture for them.

However, persistence has been distinguished from these other two features by many persons. For example, the sustained activity of the newborn human child until he is fed, or changed, is usually attributed to some nonspecific "irritation" or "drive," if you please. Such drive

phenomena (which underlie the point of view expressed by Brown, for example) set the organism in motion without determining what he shall do or can do to eliminate, initially, the source of motivation. So motivation really should make room for persistent behavior which is not "going somewhere" but only "going," like a Newtonian body in an unoccupied, frictionless world. However, in any case there is no question about the fact that the persistence of behavior is a feature that has captured the attention of most motivational theorists.

I don't wish to discuss the contributions of Peak or Atkinson in any detail--what I mean is I'd really like to, but I can't do it now; however, there are a few things I would like to point out for you because they will help make my final suggestions clearer. It is not clear from their analyses how action results from motivation, that is, how behavior is instigated. In general, I would gather from their analyses that stimuli instigate behavior, though the behavior which will be instigated and the form that behavior will have is to a very great extent shaped by motivation. If this is in fact the analysis which is being offered then truly one of the conventional properties of motivation, that of instigating behavior, has gone down the drain.

Atkinson proposes that a motive is an anticipatory goal state and, therefore, that all motives are learned. We have already referred to this in our comments about the infant suffering from lack of food who is "driven" into action in the absence of any expectations or anticipations of what may happen to him. When one discovers some of the other things that people want to attribute to the neonate, the sledding for "motivation as learned" becomes very mushy indeed; consider, for example, Harlow's proposal that there is an innate drive to see (13), or Woodworth's that there is an innate drive to perceive in general (33), or, finally, Nissen's that there are innate drives for general cognitive processes like knowing, learning, and comprehending (23). It would seem likely that an analysis suggesting that there is complex behavior which is not motivated--and surely the behavior of the

neonate is complex--just will not go over.

Another difficulty confronting the proposal is that anticipations or expectations are not ordinarily construed as the kinds of things which instigate behavior. They are dispositional or likelihood concepts about what is to happen. As we have pointed out above, it does not seem to be a property of anticipations and similar phenomena that a person do anything about them, though it is true that we would not discover them or attribute them to organisms if people did nothing.

D. How to Go about Defining Motivation

A substantial amount of thinking has gone into developing ways of defining or identifying various motivational concepts. For one illustration, we will consider Ritchie's (27) contribution to the 1954 Symposium.

In his analysis of contemporary psychological concepts, Ritchie introduces Carnap's notion of bilateral reduction sentences for defining or introducing dispositional concepts and, therefore, motivational concepts. I find much of what he says exceedingly informative. But he also gives us another lesson. Woven into his analysis is the recurring question of how to define thirst. He suggests that this may be done with the aid of reduction sentences. As a logical device, however, the use of reduction sentences does not offer us a unique way of getting what we want. Actually there are many different kinds of ideas that may be introduced in this manner. And the fact that they usually are dispositional concepts, e.g., habit, solubility, does not help much at all. As Bergmann has suggested, ". . . virtually all scientific concepts are dispositional in this sense" (3, p. 60).

How, therefore, are we to know which of the terms that are introduced as dispositional terms are motivational terms? The use of reduction sentences will not tell us. As Farber says, in his comments on Atkinson's paper, there must be some basis for knowing when you

are talking about motivation and when you are not; it must have some distinctive properties to differentiate it from, say, learning. And this is exactly what we do not get from the important insight that motivation frequently has something to do with dispositions or, more correctly, dispositional analysis, which is of course not quite the same thing.

Therefore, an analysis like Ritchie's is going to be of value only if we know on some grounds that, e.g., thirst is a motivational concept. Then we can introduce refined ways to insure the effective use of motivational concepts, to make sure that we don't get trapped in the maze world of the counterfactual conditional, and so on.

E. Internalism

If one views an organism from "outside," as is usually done, and in the same way as one views, say, a billiard ball, he will see certain things occurring in the presence of the organism and he will see the organism doing certain things. Within very narrow limits, the behavior of the billiard ball can be predicted with excruciating precision. But we all know this cannot be done for organisms given the same kinds of observations. Since we cannot treat the behavior of organisms by Newton's laws, we must look for the disrupting influences. The variation in behavior in the face of identical inputs and the constancy of behavior in the face of varying inputs leads us beneath the skin of the organism. Inside of him we shall find the source of variation. Therefore, motivation is what is inside the organism that accounts for his plastic, variable, and uniform behavior.

I believe there is much truth here, though I am not disposed to treat lightly the very real variability of input that goes on in extralaboratory life. There are two difficulties associated with this viewpoint that I must point to, however. The first is that what has been put within the skin is mostly fictitious; it is a retranslation of what we already know into more prestigious and less attackable

(but not, therefore, more defensible) language. We are just beginning to know what in fact does go on beneath the skin that is important for behavior; up to now about all we knew with any security were the phylogenetic consequences of not having certain brain structures and of having certain gross disorders of neural and endocrine tissues.

The second difficulty is this. Psychological theory has put too much beneath the skin. We have not only peopled our insides with motivational phenomena. We have housed there many other processes that we found equally refractory to simple Newtonian type laws. Even if we were to deny any physiological implications to our internalism--the sort of thing that Brunswik, for example, intended, when he spoke of "centralism"--there turns out to be so much in the central core that we cannot sort things out neatly enough to be of any help. There are, for example, capacities and habits inside. Since we don't know very well the way "inside" things work, we use outside things to help us figure it out; but we manufactured the "inside" so we could account for the outside and now we have to turn about and use the outside to clarify the inside. Or, to put it another way, we knew (note that the tense here is past) as much about motivation from an outside point of view as we did from an inside point of view when the notions of motivation were formulated. And it was surely not the act of putting them inside which made them motivational.

F. Motivational Physiology

There is a world of difference between a fictitious and real physiology. Today, it is really exciting to see what is being learned about both the complex anatomy and physiology of the neuro-humoral system and the varieties and properties of neural elements. But this is not what I want to talk about. I want to mention, and this for emphasis only, the same thing that I discussed earlier in respect to Harlow's comments about Brown. The recent work in motivational physiology is exciting because it is

giving us additional information about motivation, not because it is telling us what motivation "really" is. Rather, we know it is relevant to motivation because in some sense we already know what motivation is. Motivational processes were characterized before the neurophysiological processes were studied. The belief and knowledge we have about motivation has been achieved at a "molar" level--though, to be sure, in recent times it has too often been expressed by inferring something about or imputing something to the nervous system.

So I conclude that while the significance of neurophysiology for the scientific understanding of motivation is enormous, it is so because we relate this neurophysiological knowledge to what is already labeled as motivational. Its significance arises from the fact that a correlation is being drawn. It is for this reason that the recent analyses of Hebb (15) and Wolpe (31) in their attempts to define motives in terms of some central neurological process are misdirected; they do not appreciate the problem which confronts most psychologists in working sensibly with the concept of motivation. What is needed is some way of designating motivational phenomena so that the properties of physiological mechanisms can have some stable motivational properties with which to relate. So it will not do, logically, to define motivational phenomena in physiological terms. Whether it will do for explaining them is, on the other hand, an empirical matter it will have to contend, for example, with sociocultural explanations, but that, at least, does not involve the same kind of logical problems.

G. Awareness

The last orientation toward motivation that I shall comment on deals with the motivational phenomena of consciousness. Concepts like volition, desire, preference purpose, and so forth have historically been regarded as motivational. There is no doubt that, historically, many people regarded such motivational phenomena, or some class of them, at any rate, as exclusively conscious phe-

nomena. One need only consider the attacks upon psychoanalytic thinking in respect of motives to see this. You will recall that many of the attacks were directed against the notion of unconscious motivation on the grounds that motives can only be conscious; desire, wish, and willing are, in their essence, conscious, and that is all there is to it. It is a contradiction in terms to speak, therefore, of unconscious motives.

I think this is the result of the peculiarly esoteric and effete version of mind that the British associationists bequeathed to us. There is ample historical evidence that motivation always meant more than conscious motives or impulses. Animals, though denied consciousness, were rarely denied purposive or teleological characteristics and these, we have seen, are motivational. Men hated and loved without knowing it--how much of Shakespeare consists in pounding home this kind of moral? The "insane" knew not what nor why they did things but they were not therefore unmotivated; to do things out of madness was perhaps to do them because one was inhabited by a demon or because one was motivated by irrational impulses, but motivation there was.

Yet while it is true that motivation is not exclusively conscious, it is also true that some motivation is conscious, or to put it better, that there are motives of which we are aware; there are conscious motives. To fashion a definition of motivation, then, which denies to conscious motives exactly the same status as nonconscious or unconscious motives will eliminate some motivational phenomena which historically are entitled to membership in the fraternity. And this, I fear, is what is done by most of the definitions proposed. This is accomplished by postulating for motives, mechanisms and causal properties which are either denied to conscious motives, or which they cannot possibly possess. They are therefore rendered epiphenomenal at best or nonexistent at the worst.

II. A Definition of Motivation

Let me review now what I have done so far. I have indicated that I believe we already know in some sense what motivation is. I then reviewed a variety of attempts to define motivation and pointed out that each of them was defective (whatever other virtues they might possess) because they legislated out of the realm of motivation, concepts which were there before the definitions were constructed. Since the purpose of the definition was, in the first place, to provide a rationale for the grouping, when it proved unable to do the job it could not then be used to regroup the concepts. And this is what most of the definitions wind up doing.

I would now like to offer a "definition" of motivation that is sufficiently comprehensive to incorporate all previously listed motivational phenomena and all phenomena of that sort which might be discovered in the future. "Motivation refers to processes or conditions which may be physiological or psychological, innate or acquired, internal or external to the organism which determine or describe how, or in respect of what, behavior is initiated, maintained, guided, selected, or terminated; it also refers to end states which such behavior frequently achieves or is designed to achieve whether they are conditions of the organism or environment; it also refers to the behavior engaged in, or aspects of that behavior, in respect of its organization, occurrence, continuation, reorganization, or termination with regard to past or present or future organic or environmental conditions; further, it refers to the fact that an individual will learn or remember or forget certain material, as well as the rate or manner in which these processes occur and the ease or difficulty with which they are altered, as well as to some of the processes or conditions which are responsible for this behavior; similarly, it determines how and what perceptual and judgmental activities and outcomes will occur, as well as some of the conditions and determinants of such activities and outcomes; similarly, it also refers to the fact of and the determinants of the occurrence and fate

of affective process; finally, it describes and accounts for various individual differences which appear in respect of the various behaviors, processes, conditions, and outcomes referred to above. Motivation refers to any one or more of the above behaviors, conditions, processes, or outcomes in any combinations."

Now, I think you will agree that there isn't anything that might be called motivational which cannot get inside this definition. The only difficulty is that there doesn't appear to be much of anything else that a psychologist might be interested in which it would exclude. And that, I'm afraid, is true. So we must continue our analysis a little longer.

III. The Restoration Period: Some Biased History

In order to understand our predicament today we must realize that it is not at all new. I have already hinted at this in my comments about the disagreement between Harlow and Brown. And indeed if one looks at the literature of the ancient Greeks or the classical literature of the Romans, we can see how true this is (12). All the generic motivational concepts are there; they exist and are used with all the abandon, vagueness, and ambiguity that we are accustomed to seeing today. One even finds physiological speculations which catch the spirit if not the substance of contemporary physiological research and theory.

In the centuries after the Romans, matters did not change much. The same disputes, the same kinds of analysis and theory recur over and over again.

It is astonishing that such persistent disagreement continued as long as it did before a radical break occurred. But finally, though for a brief time, there came into academic power a point of view which neglected the traditions of motivation in regard to the mind. I refer to the latter half of the nineteenth century. During that period the academic psychology of the German and British associationists

had as its objective the development of a rigorous theoretical structure. In order to do so it became necessary to circumscribe the material they dealt with so severely that most of what had at one time appeared important in psychology became exorcised, or transferred to some other discipline.

I believe the evidence is strong that this was not done blindly, though it was wilful. Titchener--though he was late in the game--knew very well what he was about. That may be seen clearly enough in one of the most profound books published by a modern psychologist, his Prolegomena (30). But much earlier in his lectures on "The psychology of feeling and attention," he had said, ". . . the doctrine of attention is the nerve of the whole psychological system, and that as men judge of it, so shall they be judged before the general tribunal of psychology" (29, p. 173). Elsewhere in that same volume he says, "Fortunately or unfortunately, however, the main current of modern psychology takes its source from the intellectualism of Herbart and the sensationalism of contemporary physiology. Hence . . . experimental psychology had from the outset, a strong intellectual bias, a definite leaning toward 'sensory' feeling. . . . What is significant . . . is this: modern psychology, just like the psychology of the eighteenth century, has finally revolted against intellectualism, so that the majority of present-day psychologists recognize the independence of the affective processes" (29, pp. 285-287). Still further on he says, " . . . as far as I can see, the term 'will' affords the best general title for two great groups of psychological facts; the facts of attention and the facts of action" (29, p. 297). Is it not tragic that he was committed to such an austere view of psychology that he could not act productively on this insight--an insight, I believe, which was much the same as his more potent and enduring contemporaries, Freud and McDougall, had gotten hold of.

The grip of the "pure" associationist school was not to last too long and it never conquered more than a bit of the territory it sought. Let me outline briefly a number

of factors which prevented the hold of the associationists from getting too strong and which finally weakened and broke what hold they had. This review is pointed toward indicating the setting in which motivational notions returned with a tremendous surge.

1. The Vacuum

We have already indicated that the version of psychology that was academically dominant was one which had little room for motivational concepts. By the same token, what that psychology did offer in the way of concepts was not enough to handle the richness and organization of experience, nor the spontaneity and continuity of conduct. There was a need for concepts which would help solve what people generally saw to be the problems of psychology but which would do so in terms of the new knowledge and insight that modern science had achieved.

2. The Example of Physics and Chemistry

For all thinkers in the nineteenth century there was no question where the greatest prestige and success in science lay. It was in physics and chemistry. What accounted for the phenomenal success and power which these disciplines had attained? Why, it was the analysis of force and the discovery of the manifold forms of energy which were responsible. So if one could match such concepts in psychology, perhaps the same success would be matched.

Behind this lay an age-old association of motivational concepts with those of "force." While I shall treat this more systematically soon, let me point to one instance. The word "conation," which today covers motivational concepts, derives from the Latin conatus which means "effort," "exertion," or "impulse" (that's an interesting collection, is it not?). Well, in the seventeenth century, the term conatus was used to refer to physical force (this is true today, too, as a glance at the American College Dictionary will show); the great Huygens used it as the

name for the quantitative expression for force that he formulated (16). At that same time, Hobbes and Spinoza were using conatus to express the facts of human motivation. So here was indeed a hopeful path to follow.

3. The Revolution in Biology

In the nineteenth century, biology underwent several revolutions. There was, first, the morphological and functional impetus associated with the cell theory. Secondly, there was the achievement of biochemical and biophysical analysis heralded by the synthesis of urea and tissue metabolism. Finally, the functional basis for taxonomy was established with the Darwinian theory which shattered once and for all the barrier between man and animal.

These created a frame of mind in which the significance of the concepts of force and energy in physical analysis were enhanced. The fact that the mechanisms of organic functioning were apparently uniform throughout the animal world now meant that the similarities between animals and men, which had been dismissed as accident, coincidence, superficial resemblance, or the work of God, could be real, analyzable similarities; real in the sense that the same causal processes were at work. And this was extended to cover all the characteristics of men and animals. The systematic study of organismic behavior was restored to the important position it once held in ancient times. And man was now part of it for good.

So it became urgent that there be concepts which would provide for the release and control of the latent energy which all organisms possessed.

4. Mental Science into Experimental Science

As with so many things, psychology generated its own dissatisfactions. As long as experimental psychology confined itself to sensory phenomena, the primitive neurophysiology of that day--itself primarily structural--was

a splendid escape hatch. But when experimental methods were applied in rapid succession to higher mental processes like learning, problem-solving, judgment, and imagination, to the instrumental behavior of animals and people, and to the growth of the child, the inadequacy of the neural model was obvious (though not, therefore, universally dispensed with). Psychologists would just have to do justice to their data themselves; there was no salvation in a nonpredictive nervous system, or, at any rate, in a sensory nervous system. Since it was already clear, even before the Gestalt critique, that the bond or valence concepts of associationism were never going to be adequate--as they appeared to be in chemistry--the groundwork was laid for extramural concepts.

To this series of developments which arose from within the nonmotivational psychology of the day, there must be added the tradition of psychology which was non-experimental--indeed mentalistic--but which continued to stress the active nature of mind. While this variant of academic psychology existed very strongly in England, e.g., Baine, Ward, and Stout, it was also powerfully represented in America by James, the later Dewey, and the other members of the Chicago school. For these psychologists, the essence of mind was to be found in its active relation to the world, not in the mind's geographical layout.

So here were two powerful sets of forces within psychology which pushed beyond a psychology of association and sensation, which pushed psychology out of its preoccupation with the epistemological problem.

5. New Clinical Disciplines

The tremendous population growth of the Western world by the end of the nineteenth century created a need for a variety of professional custodial and therapeutic services on a large scale. So, for the first time in history, the peculiarities of persons were exposed to large numbers of trained individuals. But these were members

of a culture that valued and emphasized systematic and scientific efforts to describe and explain. It is not surprising, therefore, that the orderly and interrelated nature of psychotic and neurotic conduct should finally become evident and make observers seek explanations which accorded with the ideals and ideas they took over from physics, chemistry, and biology; after all, even in earlier, demonological times there were perceptive persons who saw the order in madness; with the living example of physics and chemistry before them, was not now the time to make rational the irrational?

The important thing, however, was that the dominant view of mind in the last quarter of the nineteenth century was one which did not have an inductive base in these exotic behaviors. As a matter of fact it did not have an inductive, i.e., scientific, base in very much beside sensory and elementary reactive phenomena. To be sure, it is reasonable today to believe in the continuity of deviant and normal behavior; but this is because we have come to describe normal behavior by terms which were devised to describe abnormal behavior. At that time, however, it was the reverse method which was attempted and it just didn't seem to work. As Freud was so careful to point out in numerous places, it wasn't an incorrectness in the Fechner Wundt tradition that troubled him--it was its superficiality, its inadequacy.

6. Social Philosophy Becomes Empirical

The relatively young science of anthropology, quickly joined by sociology, further transformed our view of man. It became clear that man's capacities and achievements were terrifically plastic. The belief in a "typical" or "average" man that was traditional in academic environments, a view held by many political economists, moral philosophers, and the early psychologists, was shattered --though the shards have not been dispersed. The rising sociology and anthropology furnished, therefore, an additional inductive base which was in its own way as dramatic as the material brought forth by the new clinical disciplines

Behavior, knowledge, belief, even the "self," did not escape the impact of these new sciences. And the ordinary psychology of the day was quite incapable of coping with the diversities of mind that were uncovered. It was not without reason, therefore, that Wundt dispaired, and consoled himself with the belief that the higher mental processes and social behavior were really so special that they could never be the subject of scientific study.

In sum, one can outline the state of psychology and allied disciplines at the turn of the century somewhat as follows: the kinds of explanations that the dominant psychology could provide were well known; a great variety of individual, social, and cultural phenomena were uncovered and systematized by people who were not primarily psychologists; they looked to psychology to explain their phenomena and they expected explanations which had the same characteristics as physical, chemical, and biological theories; but psychological explanations were unsatisfactory because either there were not any concepts of the sort that were needed or the ones that existed were incorrect.

Psychology did not possess adequate concepts to explain action. Its account of the arousal or stream of ideas, images, and percepts was dubious and embroiled in controversy; the affective life of the organism had become a shadow world to be studied only with clever brass instruments. As one might expect, it was not too great a step to question whether its description of mental contents was even credible, whether its version of the range of the mind was sufficiently comprehensive.

So, the scene was set for a psychological revolution.

IV. What Motivational Concepts Are

As a result of the various pressures for a comprehensive and utilitarian psychology, the new mass of observational material, and the obvious inadequacy of the

dominant psychologies, the intellectual scene exploded into the various fragments we know today as "Schools of Psychology." With the exception of one of them, Gestalt, they were all noteworthy for the prominent place occupied by motivation. Freud, McDougall, Watson, Angell--for all of them conative concepts were the core of their thinking And we have continued the traditions of these thinkers. Motivation has been restored to a high place in the life of animals and we have aggressively extended its application to all spheres of activity--individual, social, cultural, and historical. To be unable to have a satisfactory formulation of motivation under such circumstances is an exquisite form of torture.

In sum, the Restoration Period in psychology fixated for us, once again, the significance of motivational concepts. But it did not do so in a fashion which permits us to feel free of the long history of such thinking, nor indeed has there occurred any strikingly new conceptualization of motivation. We now have overwhelming reasons, however, never again to do what that small band of psychologists at the turn of the century did--ignore phenomena, especially those called motivational.

A moment ago I excluded Gestalt psychology. You will note also that I failed to mention the "Psychometric Schools," especially Spearman and the tradition which stems from him. What I have to say from here on out applies directly to Gestalt psychology. I think it also illuminates the very strong resistance to psychometric approaches to behavior that exists even today and makes clearer the grudging insistence upon its significance by some of its major theorists (5, 8).

What I would like to do now is to offer you a hypothesis as to why certain kinds of concepts are considered to be motivational and why such concepts are regarded as so important that they provide the basis for intellectual revolutions. The hypothesis entails social, historical, and ideological facts that I shall not now attempt to document. I have taken too much time as it is to prepare the

ground for this suggestion to try to give you an exposition that satisfies all the requirements for scholarly presentations. I believe, on the other hand, that many of you are acquainted with the sort of thing I am going to speak about, and a good bit of the material that is its evidence.

Throughout the history of man there has appeared in many guises a distinction between two different kinds of "existence." It has been in terms of this distinction that most explanations of the behavior of animate, inanimate, and even "spiritual" beings have been couched. One of these "existences" I call, for want of a better term, the active principle and, as you might expect, the other I call the passive principle. The kinds of concepts that these two terms designate appear in various approaches to behavior, whether it be psychological, social, or physical behavior. These two kinds of concepts appear whether the approaches are religious, scientific, or what might be called metaphysical. They appear in ancient times and they appear in modern times. If the terms I use sound like those that Aristotle might have used, you are correct; they are also like the terms that Aquinas used; they are also like the ones that Hobbes and all his descendents on both sides of the channel have used.

Now what do I mean by active and passive principles? An "active" is anything that does something to something else; a "passive" is anything to which something is done. All the things that are actives get collected into one category and all the things that are passives get collected into a different one. There are many kinds of things which have been regarded as actives, and their history is the history of man's attempts to explain his world and their fate is the story of man's willingness to adopt or reject them. Hence the history of "force" is an illustration of the search for a properly formulated and fruitful "active" which would apply to terrestial and celestial phenomena, to mechanical and chemical phenomena (16). And its intimacy and even identity at many points throughout history with the concepts used to explain organic behavior--remember the conatus of Huygens and Hobbes and Spinoza we dis-

cussed earlier, but also do not forget the pneuma and the entelechy of both the naturalistic and vitalistic ancients nor the impulse and drive of the modern--this intimacy and identity has been no accident; it has been an expression I believe, of just this belief in, and search for, "actives." It doesn't much matter what terms are used to designate these concepts. I could have spoken of sprites or demons, of devils or angels--each of them refers to some kind of notion of an active. Or I could have expressed the distinction in the terms of Jung's anima and animus--"maleness" refers to the active principle and "femaleness" to the passive. The Chinese speak of Yang and Yin. You see, there are many ways of getting at this idea and most of them, I suppose, have been tried. The occidental beliefs in a "prime mover" who imbued (or, in some versions, continuously infuses) inert clay, primal matter, with life expresses this. But the various polytheisms express this also. The point is, in any case, clear; some things are inert and others are vital.

It is not enough just to point to the distinction between active and passive principles, of course. What is needed is a detailed analysis to elaborate the characteristics of actives and passives, especially the role they have played in the history of ideas. But such a task involves too much logic and too much in the way of philosophical dialectics for me to be comfortable with it; I shudder when I think of the history of dispute concerning these distinctions. Therefore I shall make only some running comments, draw a few distinctions, and let it go at that for the present

What I will try to point up in my analysis is the following. It makes perfectly good sense to call some things passive and some things active. But it does not make good sense to think that there is anything which is, in some sense of the term, "absolutely" active or passive. Therefore, anything--as far as we are here concerned this means any event or condition that we might wish to call psychological--may be an active or a passive. In other words, to be an active or passive "agent" is relative to some framework, and as the framework changes so

does the status. Therefore, what is motivational to some people may not be so to others. Because, you see, I am arguing that "to be motivational" is to be anything which may be regarded as "active." And that is the answer to why list number II is such an odd collection; it is the historical collection of concepts which have been regarded as "actives."

A. Etymology

The first thing to point out in considering this hypothesis is that etymological arguments are completely irrelevant. For one thing, as I suggested earlier, the root meanings in the Greek or Latin or Old English are already ambiguous; and this is so because the distinction being considered is at least as old as the languages we use to express it. For another thing, to be active or passive is a very abstract property--it is not to be treated as synonymous with, say, motility or lack of it, though to be sure one may analyze motility in these respects. As an abstract property, the meaning of active or passive does not entail which particular is to be classified as active or passive; whether this or that particular is active or passive does not follow logically from the logical fact that it must be one or the other. Which way an "item" (conceptual or observational) is to be classified depends upon who does it, or, more properly, the framework in terms of which it is done. And as you well know, the variety is legion. So it is evident that if motivational concepts are the actives of psychology, anything can be a motive; and if this is so, then it is inevitable that there will be conflict as to whether any particular item is "truly" a motive, since classification involves some form of proposal or persuasion; but because this is so, I hope you will see that it is not really very important that something is a motive--what is important is that we can conceptualize its properties.

B. Some Historical Aspects of Psychological Actives

The psychologist's list of actives is not something which any one person has constructed, just as it is not

a contemporary invention. This was the notion lying behind my tedious strategy of pointing out that "so-and-so proposes such-and-such as a definition of motivation, but look! He is not admitting these and those which are already in the category of motivation." To be motivational is to be part of a historical and social legacy. As with any legacy, however, there must have been some work and tribulation associated with it, and therefore what we have at any time as the list of motivational phenomena are those concepts which have survived dispute and analysis. We have a socio-linguistic category, in other words.

Here is an illustration of what I mean. It is an illustration which teaches us an important lesson about what is currently meant by most psychologists when they adopt the notion that "habit" is a potentiality, a something which requires drive (an active) to result in behavior. While this sort of thing is identified most with Hull because of his famous string of formulae leading to momentary effective reaction potential, it is also true of Tolman, of course, and appears in Lewin, who, admitting associations (habits), insists that "the couplings created by habit are never, as such, the motor of an event" (20, p. 43). Habits are, therefore, one of the kinds of things that people today agree are passives. But as I will show below, this need not be so (see "I"), and that is why Brown and Farber have so much difficulty with the class of things called "learned drives."

But back to my illustration. You are all familiar with the school of psychology that was named the "Faculty Psychology." The mind was endowed with the capacity to carry out certain functions and these were referred to as faculties. While doctrines varied, the generic usage of the faculty as something which could be invested and utilized by a unitary soul declined and we were left with faculties which possessed their own motive power, so to speak. It is this particular usage of "faculty" which for a while in the seventeenth century (and in the thinking of many laymen today, I believe) would have gotten "faculty" onto our number II list. But it got beaten down and "we"

no longer regard faculties as the kinds of things which could be actives--even if they existed. So the psychologist's list of motives is a residue out of preceding historical events and does not give us a unique basis for extracting a generic property; history has created so many gaps that our contemporary list does not contain enough information. It is only when we regard the problem historically that we realize what has happened. If we had taken seriously such expressions as "force of habit" we would have appreciated all this sooner.

C. Problem of Action

One property that comes to mind when thinking of an "active" (isn't that a horrid noun?) is of something which gets around. A moving billiard ball is an active; so is a tumbleweed; so was the apple as it dropped past Newton. But there are some things which do not get around that we may also call "actives"; I refer to items which have latent or potential activity as a property of their present state. So it is that a spring which is not compressed is a passive (usually, that is--I must be consistent) but one which is compressed is an "active"--if anyone doubts this, let him watch the precautions in an automobile repair shop when a coil-spring front wheel is to be removed, or, if he wishes to stay at home, let him try to reupholster an easy chair. This is nothing but the old physical principle of kinetic and potential energy, both of which are ordinarily construed as "actives."

Well, if we can find actives in the physical world which do not "get around," then it should be easy to find them in the behavioral world of psychology. And as I took pains to point out in my comments on the various proposals for defining motivation earlier, psychology is loaded with them: tendencies, inclinations, proclivities, dispositions, sets, determining tendencies--need I go on? Let me say only that these very illustrations mean to you what I think they do because of the framework in which you regard them. Change that a bit and the fluff is out of the souffle, the cake has fallen, the easy chair has collapsed. Let me try this

one on you; is a "trait" a passive or an active? (Don't let the "level" of the language fool you; actives and passives occur at all semantical levels, I believe.)

D. Durability

It is possible that an item must be an enduring active? Trivially, of course, we have already excluded this by indicating that actives and passives are always relative to some conceptual framework. But that is a logical distinction, and I am referring now to a nonlogical or existential distinction. But you can see, immediately the question is raised, that this need not be true. For consider, ordinarily hunger is an active, and when a person is hungry it will alter his behavior in some determinate way, just as it will alter or participate in the alteration of a particular habit (which is usually a passive--recall Brown's analysis). Nevertheless, a person may be hungry but his behavior will not be guided by that because he also has a full bladder. At this point, my suggestion meshes with the analysis proposed by Ritchie, because I am saying that the person is still hungry but that now hunger is not an active. Nor do I think that we can call it a latent active in the sense that a compressed spring is, because the spring exerts a measurable force upon the clamp, the "set" tells us whether the subject will respond to the pretty female experimenter or the ordinary male experimenter. So an item may enter and leave and return to the status of an active, though there has been no change in the basis for believing it still exists; therefore, actives are not permanently so.

Note now, we have not been talking about the case in which an active has a life span (as hunger has) so that, when the organism is requited, the hunger dies. Of course hungers come and go in this sense, as do most motivational actives; we have been talking, rather, about this case: when given an item, ordinarily considered an active, which has not been requited, must it necessarily be considered an active? I suggest "no" because otherwise we are forced to depend upon a fundamental circularity in defining our

concepts; to insist upon behavioral evidences at every stage in the life of the item, e.g., hunger, is to say that a motive may be inactive or inhibited when it isn't necessary to do that. What we mean to say is that hunger continues even if the organism does not eat. Thus hunger is not always an active and this means it is not necessary to think of hunger (even when present) as always being a motive.

E. Origin

Is there any indication in the literature that actives arise or have their origins in any particular set of circumstances? I think not! There is no question, however, that many different persons have attempted to argue for a very limiting approach to motivation in its role of psychology's prime active. On the one hand there are the biogenetically rooted theories like Freud's and McDougall's which attribute species or phylogenetic sources to drive or instinct. Here the organism is born with the basic springs to action, and even the steering mechanisms are genetically or constitutionally controlled. This is in sharp contrast to the position taken, for example, by McClelland and his colleagues who speak of motives as being learned, as being anticipatory mechanisms developed in conjunction with affective conditions.

I do not believe that there can any longer be serious opposition to the proposal that some of the motivational features of behavior are innate. There are sufficiently clear-cut species differences -- the stalking, visual cat versus the trailing, olfactory spaniel--to make clear that what will prompt an animal to action and how and by what he will be steered is not to be found exclusively in the experiential history of the animal. But it is also true that there are acquired motivational phenomena that we wish to impute to animals; the character of their behavior makes quite clear that a great deal of it is experientially controlled, in the same sense and with respect to the same features of behavior that lead us to posit innate mechanisms.

Now while it is desirable for some purposes to segregate motivational phenomena into learned and unlearned classes, this must not be permitted to overshadow the fact that the motivational notions have been developed to handle facts of the same kind; it is only that in one case there are biographical details of the individual which are relevant and in the other case species biographies which are important. So I see nothing in the various motivational phenomena to suggest that an intrinsic property of this kind of active is the fact that it is innate or learned.

F. Causal Efficacy

The most important feature of the distinction between actives and passives is to be found in the very way in which they have been distinguished. In saying that an active is "anything that does something to something else," what I have done is to stress that the distinction is with respect to causal efficacy. Therefore, it might be expressed something as follows: "Certain things possess an active principle; anything which does is a cause; motives are active principles; therefore, motives are causes."

This is surely the main feature of motives. But matters have not stopped here in psychology. What I think has happened is something like this: It is assumed that only the sorts of things which now appear on our motive list belong there. (Remember, however, the fate of the "faculties.") Anything which does not is not an active principle and therefore cannot have causal efficacy. So only motives can be causes. This takes the form of a rule or demand along the following lines: "When I want a causal explanation of psychological phenomena (and there is no other kind worth considering), the only kind which will be satisfactory is one expressed in terms of a motive. All other attempts at explanation are illegitimate because they do not reveal the real causes (or actives) at work." Put in a symbolic form this becomes

> "P causes Q (and P is at least a motivational state of affairs)"

This is the type of program which is advocated, for example, by psychoanalytic thinkers in their less sophisticated, or should I say more evangelical, moments. It is the kind of thing which appears in relation to reinforcement theory as it has developed along Hullian lines. For example, in his monumental volume on experimental psychology, Osgood, in discussing some controversial issues in psychology, considers the question of whether reinforcement is necessary for learning. He concludes that for the opponents to demonstrate ". . . the proposition that learning can occur without motivation, we must be able to specify absence of motivation, but this is not possible with our present techniques of measuring motives. The entire proposition appears incapable of disproof" (24, p. 414). Again, "Although, to be sure, no reinforcements were deliberately given by the experimenters, we have no guarantee that all forms of reinforcement were eliminated. We are in no better position to index precisely absence of reinforcement than absence of motivation" (24, p. 415). But it is obvious that if it is impossible to disprove the statement that there can be learning without motivation, it is equally, i.e., logically, impossible to prove or demonstrate that there must be motivation for learning.

So this particular argument in psychology turns out not to be an empirical one, at least in the form that it has usually been considered. And it is not an empirical argument, because the proponents of Hullian type reinforcement theory have said, "There must be some motivational factor to be reduced and hence serve as a reinforcer wherever there is any learning and we shall postulate one whenever we cannot demonstrate that there is one on the basis of our initial experimental operations." In other words, there cannot be behavior, learned or otherwise, without a motive or drive because to be an item of behavior entails being the result of some motive. And whatever it is that can be shown or has to be postulated to entail such behavior is designated a motive.[2] This, I believe, is the

[2] I owe this particular way of putting the problem to Dr. K. J. Lambert.

kind of consideration that leads psychologists to postulate "drives to know," "drives to perceive," and so on. What appear to be empirical assertions are actually the expression of logical distinctions: the characteristics of the behavior of organisms are divided into actives and passives; for anything which is a passive in a particular framework, there must be an active, i.e., a motive or a reinforcement; ergo, there can be no unmotivated learning; there can be no response increment or decrement without reinforcement (or tension reduction, etc.).

Now, I am not saying that the kinds of things which we ordinarily regard as motivational do not have causal properties; nor am I saying that motives are not among the most important class of causal phenomena that we know. What I am saying is that, even in the conventional schema that is current today, it is not the case that only motives have causal properties, and it is also not the case that motivational causes are the only important ones. I am also implying that it is possible to have behavioral systems in which the kinds of things that we treat as motives do not appear at all as causes or actives, but as passives. Indeed I believe we have begun to see that already

For example, if one looks at G. Kelly's Psychology of Personal Constructs (18) he will find a "motivationless" psychology. For Kelly, the actives in his system are the sorts of things that ordinarily would be treated as passives; surely a "personal construct" is the kind of thing we regard as inert, is it not? But, aside from the merits of Kelly's system, it is erroneous to assume that a "personal construct" must always be treated as a passive, because passivity or noncausality is a relational property; depending upon the logical system in which an item appears, it may function as a passive or an active (see "I" below).

Consider an illustration along somewhat different lines. What shall we take to be the instigator or other kinds of causes of motives? To be sure, some have argued that when you get right down to it motives are physiological mechanisms; but in the same sense, so is any other kind

of psychological process, so there is no gain in that direction. We must therefore be able to conceptualize the causes of motives either as being also motives (this is often done by arranging a hierarchy or chain of motives which are never exhausted before the organism dies) or else as not being motives. But if they are not motives, are they to be considered as psychological actives or not? The subtleties of analytic thinking in respect of ego-motives or those of the Hullians in respect of secondary motives suggest to me that this is not an easily solved dilemma; what has happened is that ego-items or habit-items turn out to have properties that they are not supposed to be able to have.

As still another illustration of the distinction between actives and passives, consider the following kinds of expressions which appear in criticisms of various theories: "undynamic," "static," "mere correlation," etc. Expressions of this sort are used, I believe, to indicate that the kinds of relations which are being studied do not have the capacity to produce, lead, guide, or direct conduct; they do not have, in other words, causal efficacy. In contemporary times, critiques of this sort usually involve the introduction, somewhere in the system, of motivational concepts, but not always. The Gestalt theory, for example, has not developed a theory of motivation corresponding to those of the different kinds of functionalists, whether they be of the Jamesian, McDougallian, or Freudian variety. But they certainly have introduced concepts designed to play the same role as motivational concepts. This can be seen in the way that the course of behavior is explained with the use of concepts like "tension systems," "valence," "dynamic field," "trace communication," "Prägnanz," etc.; these are scarcely the sorts of things that most psychologists use in working with psychological behavior, but there is little doubt that such concepts are the Gestalt analogues of the functionalists' needs, purposes, drives, etc. Both sets of concepts are the actives of the respective kinds of systems, so it would be foolish to criticize Gestalt psychology because it is "motivationless."

I have not said most of what needs saying on this score. In initially thinking about this paper, I developed the argument that whatever functioned as a sufficient condition in most psychological systems became "its" motivational construct. However, this particular formulation proved too limiting. Further analysis led me to the notion of active and passive principles. The flexibility of these latter two constructs permits drawing a much broader picture of motivation, especially in respect of its historical character, its relations to ideas in other disciplines, and its ability to provoke endless arguments in psychology.

However, I still believe that the underlying significance of actives and passives is in their imputed causal character, and that motivational controversy arises because of different understandings or beliefs about the analysis of causality. That is why I would have liked to devote more time to presenting such an analysis. In point "I" below on "Convenience" the thread of causality is picked up again to some extent. But the matter is still quite vague. I hope, however, that when I am finished I will have said enough to show you what a more thorough analysis might be like.

G. Mode of Operation

In the various discussions of motivation that we have seen in these Symposia, perhaps the greatest disagreement has concerned whether motivational phenomena are to be treated as energizers, directors, or selectors. The amount of time that I found necessary to deal with this problem is as good an illustration as any, I suppose. Because of that extended discussion there is little need to go into the question deeply here. Let me indicate briefly, therefore, the effect which actives may have upon the passives of a system. When the problem is posed in this way, I am sure you can see that there is no reason to regard actives as restricted to having any one kind of effect. In the psychological systems which ordinarily hold sway, it is necessary to have something which energizes or prompt into action the passives of the system. But this is quite clearly the result of our particular formulations. Angyall's

(1) biosocial system of personality and Stern's (28) personalistic orientation both contain concepts which are passives but which nevertheless are characterized by the fact that they are activity patterns. There is no need to seek energizers for them because they are defined (by postulation) as being permanently in action. I know this sounds like a self-contradiction, but let me assure you that such feelings are more informative about the culture of psychologists than they are about the world they seek to explain.

Physics provides us with an instructive instance: Newton's first law wrested motion from the grip which the concept of force had got on it and made of motion a physical passive (6); the subsequent fate of the Newtonian accelerative force schema under the impact of the criticisms of Mach and Hertz (16) is, if anything, even more instructive. So with such an authoritative illustration before us, I suggest it is clearly unnecessary that there be psychological actives to set behavioral systems into motion; systems may be so constructed that they already have the property of motion or activity. It is the failure to recognize this that forces Brown and Farber to treat organic behavior in a pre-Newtonian fashion, as requiring always some impressed force to set it into activity. While it may be more difficult to conceptualize, it is equally true that the directive or selective features of behavior can have the status of passives in some systems.

H. Animism

Is the concept of actives an animistic one? To this question I would also answer, "No!" But it often has been animistic. While the earliest Greek and Ionian philosophers were exceedingly physicalistic in their account of organismic and physical behavior, this was not true for their ancestors or descendants. We need not consider such historical matters in detail; they have been recorded in most histories of philosophy. Nor is it necessary to do more than point to the highly spiritualistic character of most religious thought to see evidences that animistic ex-

planations have been common; both the soul and the mind have been seriously considered as insubstantial entities having causal efficacy that inhabit or invest a body. While it is very common among nonliterate peoples (19), it is clearly not a primitive way of thought unless one wishes to consider anyone who entertains animistic notions as primitive.

There was a clear-cut "fear" of motivational concepts because of their animistic potentialities during the first quarter of this century. The German objectivist school with its preference for tropistic concepts may be understood in this light. The objections of Skinner to treating drive as a stimulus condition as well as his objections to admitting other sorts of theoretical constructs are, I believe, also reflections of the fear of animism. And indeed the loose ways in which these concepts have been used do not make such an interpretation untenable. However, I believe that much of the language is exactly what it appears to be--metaphorical, figurative, and elliptical--a way of saying things which can be said without the flavor of animism, without implying that there is some inner man or spirit who activates and guides our conduct.

Therefore the concept of an active is more general than that of animism. This is so because an active gets its status from the empirical or imputed relations it has with passives. For that reason, while many of the things which are actives may be also interpreted in animistic terms, this is not true for all actives. For example, in the system of Skinner, if one asks why an operant appears with a particular frequency or rate, the explanation will be found in the reinforcement schedule that controls it. Relative to Skinner's system, reinforcement schedules are actives. To be sure, we are not tempted to consider reinforcement schedules as motivational phenomena, except perhaps in the case of self-administered ones. But I can easily imagine a time when they could. In any case, though the logical relation of reinforcement schedules to behavior is that of an active, it is obvious that it cannot be sensibly treated as an animistic concept. Therefore,

while all animistic notions are actives, the converse is not true.

I. Convenience

It is probably apparent to you by now that the relationship of active to passive is primarily one of convenience. This is not true, of course, for animistic or anthropomorphic actives; here there is indeed a mover and a moved. But if you will consider the relations among contemporary scientific concepts which are not infused with animistic qualities, the properties of primacy and priority, for the most part, vanish.

There are several reasons why I call this a "convenience," but I shall discuss only two of them. First, scientific analysis cuts into the flow of events, or segments the structure of relationships among events, in an arbitrary manner. That is not to say that it does so unintelligently, for there are good reasons; certain phenomena are more interesting, or more significant socially, or more salient because of the knowledge and beliefs existing at any given time, and it is in relation to such phenomena that we frame our thinking.

For example, because we are concerned with controlling the world about us, we structure organic behavior--some of us do, anyway--into stimulus-response sequences. However, there is little doubt that the early suggestion of Dewey (7), and others since him, that what is a stimulus for one sequence is a response for some other, is often true. And this being true, we could cast our knowledge in R-S terms once we know the laws. However, if we did we would lose much, if not most, of the control that we want. So stimuli become "causes" and responses become "effects"; yet it is obvious that we could just as readily construe them so that responses would become the things called "causes" and stimuli the "effects." Two distinguished analyses of behavior based on the recognition that they involve cutting into the flow of events, those of Kantor in his concept of the behavior segment (17) and Muenzinger

in his notion of the TE unit (22), illustrate this point quite clearly. Once such a framework is set, then the structure of our language, so to speak, dictates what gets called what. I admit the mind reels under the strain of contemplating such a bizarre possibility; but that only says something about us, not psychological theory.

The second reason can be understood by considering a skeletonized version of Hull's famous equation: ${}_{S}E_{R} = {}_{S}H_{R} \times D$. We ignore the full equation for momentary excitatory reaction potential for convenience since the argument would be the same. Ordinarily this relationship is interpreted to mean that there is no behavior unless some values greater than zero exist for both ${}_{S}H_{R}$ and D. Thus if D equals zero there will be no reaction regardless of the value of ${}_{S}H_{R}$; by the same token, however, if ${}_{S}H_{R}$ equals zero there will be no reaction regardless of the value of D. However, we work with situations where there is always some ${}_{S}H_{R}$ so that, given a D, some reaction must always occur. But this appears to be so only because we ordinarily hold ${}_{S}H_{R}$ to less rigorous standards. We can always (we think) control the D state of an animal, so usually it is D which is manipulated. Further, in the non-laboratory life of our subjects we are usually able to infer (only in a qualitative way, to be sure) more reliably what kinds of ${}_{S}H_{R}$'s they possess at any time. Therefore, it is necessary only to introduce the appropriate D's (and other components of the total equation) in order to get a response and it seems, therefore, that D is our active.

However, you can see that though this is a convenient and realistic way to look at things, it does not exhaust the logical possibilities. We need only to consider the detailed characteristics of behavior to see why the usual analysis of D as the active is faulty. If we have a given D, the resultant behavior depends upon the particular ${}_{S}H_{R}$ and its specific value. And what I mean by "depends" is simply that ${}_{S}H_{R}$ activates the total system to yield a response of a particular character; if everything else is equal, then the response is uniquely and totally dependent on ${}_{S}H_{R}$. It is only an accident of our technological ignoranc

which makes us look on D as the active portion of our equation. If we could--as someday we may--systematically alter neurological structures on which ${}_{S}H_{R}$ probably depends, we would have a situation in which we could elect to control behavior either by manipulating D or "${}_{S}H_{R}$" without going through the extensive training procedures producing ${}_{S}H_{R}$ and which make us regard it as an inert, passive residue.

Therefore I suggest that actives and passives achieve their status because of the various interests or control facilities we have at hand. And these are matters of convenience from the standpoint of theory no matter how much awkwardness or practical difficulty such a view may entail.

V. What Should Be Done with "Motivation"?

Let me review what has happened. I began by raising the following question: "Do all the things which have been called motivational have anything in common?" I reviewed a number of different kinds of definitions or analyses of motivational phenomena and argued that none of them was broad enough to incorporate all the things that were regarded as motivational. Then I presented a definition which was general enough to incorporate all motivational phenomena; by this time we were all in on the joke and it was clear it would not do. A brief historical excursion reminded us that motivational concepts were both very ancient and not too different from modern ones; we also saw that in modern times motivation has come to play such a privileged position in psychological theory that we could label our present period as The Restoration. We have just completed a section in which I suggested that the thinking of people (scientists and nonscientists alike) about phenomena ordinarily involves dividing the world into active and passive components. I then argued that most people have come to regard anything which plays the role of an active in explaining conduct as motivational; hence, that the thing which all the motivational terms have in common is their capacity to function as actives in psychological theory. In a brief analysis of the concepts of

active-passive, I have shown that there is no foundation for treating anything as absolutely an active, that to be an active is relative to the logical properties of the theoretical system in which it finds itself. This is where we are now.

Is there anything further which can be said? I believe there is much, but I shall try not to say too much. I believe I have shown you the basis for the grouping together of the various motivational terms in list number II. You are entitled to ask, however, "So what?" And I would say, "Not much."

For consider, why is there so much time devoted to formulating "definitions" of "motive" and the hundreds of other terms which might go into our list? Obviously, the purpose is to characterize their generic properties and then spell out the behavior laws in which these properties are involved. If in this sense, all motivational phenomena are unitary, that is, if they all possess substantially the same properties, then if one elaborates the laws for one of them, he has done so for all of them, and if a physiological or sociocultural explanation of one of them is provided, then one is provided for all of them. And this is indeed a glorious objective.

But this glorious objective is an illusion, a phantasmagoria of unrelated phenomena, causal relations, and consequences. For if my analysis in terms of actives and passives is acceptable, then the various concepts which have been proposed as motivational have in common only the trivial fact that they are psychological actives, that is, that they are regarded as having causal efficacy. "Well," you might say, "isn't that important?" I would not deny that it is, but I would rush to point out that to have causal efficacy is a purely relative matter. So what may be thought of as playing an active role in behavior has that complexion only because of the framework in which it appears; in a different framework it loses its vigorous, ruddy character. Therefore, the least important thing one would want to point out about a scientific concept is

the fact that it plays a causal role.

It might appear as a result of this analysis that we might just as well dispense with the concept of "motivation." That would be using bad judgment, however. This is apparent once we realize that the term "motivation" is quite like the term "furniture." And just as we would not dispense with this latter term because one does not sit on furniture, but only on chairs, or stools, or couches, so we should not dispense with a term like "motivation." What is important to know is what kind of concept it is.

But still you are entitled to expect more than just an exercise in grammar. What are the concrete implications of all this for the study and understanding of motivation? They are really quite simple and already known. For the implications reduce to a single formula: There are many different kinds of motivational phenomena. It is simply not the case that the analysis of one kind of motivational phenomena provides us with the analysis of all, or even a substantial portion, of the other motivational concepts. Motivation is not a unitary phenomenon in the sense that all motivational things have the same properties and enter into the same laws in the same ways. They do not have common origins, because some of them are phylogenetically determined while others are based upon the particular experiences of individuals. Nor is it likely that their physiological underpinnings are the same because different motivational processes undoubtedly involve different neurohumoral systems. Further, since some motivational concepts refer to tendencies while others refer to now-happenings, and only some of the tendency statements are summary statements of now-happenings, motivational phenomena are also of different logical types.

All this has been recognized by many psychologists and accounts for why there are groupings of motivational concepts. Appetites do not behave like deficits, innate motivational phenomena do not behave like acquired ones. Encephalized motives are not like brain stem motives. Intentions do not function like wishes. And so it goes.

For each of a related group of motivational phenomena we shall have to find their own properties and laws. And that is all there is to it.

Now, why is that all there is to it? Have we no hope for a master schema that will encompass all motivational phenomena? I don't believe so and I don't for the reasons I have already listed above. For we have only to consider what the circumstances for advancing motivational notions are in the first place to see why this must be so. They are introduced when any of the following three conditions obtain: (1) we are unable to render a satisfactory account of behavior or experience on the basis of the information we have at any time or foresee in the future; (2) we introduce them to summarize sets of regular behaviors which we have observed over a period of time; (3) we wish to state the causal relations between a set of initial conditions and some behaviors. Motivational phenomena, therefore, are constructions, and it can be seen that they are not of the same logical or psychological type, since they are devised to extricate us from difficulty in situations which are not all alike.

Hence motivational phenomena are like oranges, pussy cats, chairs, kites, and genes; they can all be viewed as capable of doing something, but they all do it in different ways, and for different reasons. So, the final moral is that psychologists should do as they have been doing--determine what the properties are of the things they want to study and ascertain what their laws of interaction are. That they also feel constrained to call what they study "motivational" should not be construed as saying very much, if anything, else about it.

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UNCONSCIOUS PROCESSES

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Like Professor Lindsley at last year's Symposium on Motivation, I would like to preface my remarks by disclaiming any status as an authority on the topic of motivation and to point out that I'm not sure how relevant what I have to say is to the topic of this symposium. I'm afraid the similarity of Professor Lindsley's paper and mine ends here. Professor Lindsley went on to give an excellent and highly pertinent resumé and integration of the neurophysiological centers and processes underlying motivated behavior. My principal concern in this paper is with unawareness, or, as it is frequently termed, the unconscious. More specifically I am concerned with the characteristics of unconscious processes as revealed in laboratory studies and how these characteristics are similar to or dissimilar from the processes that are termed conscious.

One can rationalize such a choice of topic for a symposium on motivation by noting that the unconscious has an important though indirect relation to human motivation. In personality study the treatment of motivational processes is markedly confounded with implicit as well as explicit assumptions concerning the unconscious and defensive mechanisms. One might also note the current interest and activity among the hucksters of Madison Avenue over the possibility of being able to manipulate the buying habits of a large segment of the population through advertising that appeals to or is directed at the subliminal or unconscious level.

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In currently popular theories of personality the concept of the unconscious has an important status. It is used to account for phenomena ranging from the formation of symptoms in mental illness to the supersensitive discrimination of threat which triggers off defensive mechanisms. At times the unconscious is portrayed as though it were a separate person within the individual capable of diabolical cunning in plotting against conscious parts of the personality.

Not only is our current knowledge of unconscious processes inadequate to evaluate their capability of carrying the burden assigned them in personality theorizing, but our knowledge is insufficient even to evaluate the feasibility of subliminal advertising. A psychologist would be hard put to cite convincing experimental evidence to show that subliminal advertising is not feasible; or, granted that subliminal perception was possible, to answer the question as to whether subliminal appeals would be more effective than conscious appeals.

The experimental literature on unconsciousness or unawareness is large but quite scattered. It is characterized by isolated studies distributed over the past sixty years, performed by different investigators with different definitions and criteria of awareness, and with widely varying experimental operations and methodologies. It has had a very scattered reception by the rest of psychology, being ignored by the toughminded and being uncritically accepted and overinterpreted by the more tenderminded. It is partly my purpose in this paper to bring together certain representative laboratory studies on unconsciousness or unawareness in an attempt to evaluate and consolidate what we know about the nature and characteristics of unconscious processes and how they differ from conscious processes.

Problems of Definition

Before turning to the main problem it is necessary to digress briefly to consider some problems in defini-

tion. As is generally the case when ignorance is vast and knowledge is small, there are but imperfect definitions available for what is meant by consciousness or unconsciousness, awareness or unawareness. Not only are the definitions vague and operationally inadequate, but there are several different terms that are used, sometimes to designate the same phenomena, at other times in a partially overlapping way, and, on still other occasions, seemingly independent of one another. Such terms as "consciousness," "awareness," and "insight" have all been used to describe the same phenomena and each in turn has been applied in its own unique way.

The terms "consciousness" and "unconsciousness" are used to designate the physiological state of alertness of an organism as well as the more philosophical problem of the awareness of one's awareness. It would seem preferable if the term "consciousness" was restricted to the physiological meaning to designate the degree of alertness and activity of the organism, and the term "awareness" to designate the psychological quality. However, the long philosophical history of the term "conscious-unconscious" as well as the psychoanalytic influence on the usage of this terminology would mitigate against any general acceptance of this distinction. Nevertheless, I will try to use these two terms in this context in the remainder of the paper.

The most frequent definitions of awareness and unawareness are in terms of verbal report. Awareness is equated with the ability to verbalize and unawareness with the inability or lack of verbalization. Freud in his extensive writings can be found on occasion to have defined awareness and unawareness in this manner (24). Such a distinction has been popular with other writers on the subject, with Dollard and Miller (14) being most explicit. They define unawareness as the unverbalized or the unlabeled, and their distinction between aware and unaware processes follows from their conception of the role of verbalization in the individual's adjustment. Verbal labels are assumed to increase the precision of discrimination and to mediate the transfer of training

through the cue-producing characteristics of the labels.

It is to be noted that a definition of awareness in terms of verbal report allows for specification of various degrees of awareness. For example, in experiments on learning without awareness it is possible that the subject may be unaware of a) the cue-producing particular responses; b) the particular responses that he is making; or c) the contingency between the cue and his response or between the response and the reward; or he may be unaware of all of these factors.

The popularity of definitions of awareness in terms of verbal report is understandable. They create the illusion of operational precision and help to hide the fact that many of the most important characteristics of awareness are avoided by the definition. There are many objections to such a definition, some of them minor and some quite serious. Among the more minor objections is the problem of just what effect the questioning of a subject about awareness has upon his awareness per se. The usual technique in studies of awareness is to interrogate the subject at the conclusion of the experiment concerning his awareness. It is quite possible that the subject's performance may have taken place without awareness, but the focusing of his attention upon these factors via the questioning may lead to awareness at this time. Conversely, it is possible that at the time of the response the subject may have been aware, and, if interrupted at that time and asked concerning his awareness, he could have demonstrated it. At the time when customarily interrogated, however, he may fail to recall his behavior or the cue--now a problem of forgetting rather than awareness.

A more serious objection to this definition involves the question of images. There is nothing profound in noting that language and words are highly abstract symbols and bear little or no physical resemblence to the objects, events, and relationships that they denote. A definition of awareness in terms of verbalization places

a heavy burden upon the adequacy of language to reflect the richness of perceptual experiences and images. The equation of awareness with verbal report leads to a similar kind of confounding of response and concept that has occurred with perception and the responses from which perception is inferred (Garner, Hake, and Eriksen [26]).

There is also the question of how we will handle "voluntary" responses other than verbal report. My verbal description of my wife might not enable you, with very much success, to pick her out from a hundred other women of a comparable age group. Yet with a mediocre artistic talent I could draw a sketch of her that would vastly increase your chances. Because of my inadequate verbal discription, a definition of awareness in terms of verbal report would lead to the conclusion that I was not completely aware of what my wife looked like. But on the basis of the drawing technique or in the case of my identifying her when I see her, I for one would hesitate in saying that I was unaware of her appearance.

Such a definition of awareness also excludes thorough consideration of a large body of experimental data that has supposedly dealt with the problem of awareness. As Adams (1) has recently pointed out, one of the most readily demonstrable experimental arrangements for showing unawareness is the following. In determining the absolute threshold for a stimulus it is generally found that if the subject is instructed or required to guess, a lower threshold is obtained than when the subject is instructed to report only when he perceives the stimulus. Significantly better than chance discrimination is found under circumstances when the subject reports that his response is a pure guess. Since the subject is making a discriminated verbal report, albeit with the subjective feeling of pure guessing, one would have to classify this as discrimination with awareness. It is interesting to note that if we exclude this area of research from consideration we will be excluding a substantial body of the experimental support that frequently has been cited to substantiate theories that hinge upon the ability of the organism to discriminate

at levels below that of awareness.

I have raised these objections and problems concerning the definition of awareness, not because I have a solution for them or a better definition to offer, but solely to insure that the issues raised in the remainder of this paper will be evaluated in terms of the limitations of our current definitions. I believe that in view of our present knowledge in this area a definition of awareness in terms of verbal report is our only working alternative. But in so defining the problem it is imperative that we constantly be aware of the limitations of our definition.

Discrimination without Awareness

The question of discrimination without awareness is in fact two separate questions with quite different implications, although they are seldom separated. When we talk about discrimination without awareness, we might mean a) is there a more sensitive (accurate) discrimination that occurs at levels not available to awareness or b) are we aware of all the discriminations that we make? The first question concerns the possibility that our nervous system differentially responds to stimuli whose intensity is below that where a differentiated or discriminated verbal report can be obtained. The second question concerns the possibility of differential responses to stimuli above threshold, the subject, upon questioning, being unable to state that he used these particular stimuli as cues for his responses. These two questions are seldom distinguished and it is not uncommon to find an experimenter arguing the affirmative answer to the first question by citing experimental data that only demonstrate the latter.[1]

[1] In discussing discrimination without awareness in this paper, I will be referring to discrimination of cues that arise outside of the person. This excludes such various physiological regulatory functions as the barostatic reflexes and chemocepters in the aortic arch that regulate heart rate and are never available to awareness.

The question of a supersensitive discrimination system that lies below what is obtainable in awareness has come into prominence recently, primarily as a result of the experimental work on perceptual defense and the more general work on defensive mechanisms. Implicit in psychoanalytic theory is the assumption that the "unconscious" is capable of perceiving threat prior to the conscious perception of the threat. Blum (7) has extended this implication to the perceptual defense problem. He has assumed that the "unconscious mind" is capable of perceiving the threatening nature of stimuli at levels below that available to awareness or the conscious mind, and that this unconscious perception of the threat is the factor that activates the defensive process. A similar assumption has been made by other experimenters who have worked on the perceptual defense problem and I myself (23) during my misguided youth favored such an interpretation of the perceptual defense data.

When discrimination at levels below awareness is explicitly considered, it is often assumed that the discrimination occurring at these levels is only of a rather gross type. Cason and Katcher (10) give a very plausible neurophysiological speculation in terms of how such subliminal stimuli could operate. They state, "...before a conscious activity comes into existence, the energy that is produced or liberated in the body must be a certain required strength, the nervous energy frequently leads to some bodily effect even though its strength is not sufficient to produce conscious activity. It seems reasonable to believe that energy of this subliminal strength can be connected with some response by the learning process, and if this could be demonstrated the theoretical implications would be important."

Lazarus (36) in defending his interpretation of the subception effect has suggested that discrimination at levels below that available to awareness, as measured by the GSR, is capable of making gross discriminations between threatening and nonthreatening stimuli. Osgood (42) has made a similar suggestion for interpreting per-

ceptual sensitization and defense data. He conceives that an over-all affective evaluation occurs early in the perceptual process and "that hints provided by the incomplete information in the stimulus display, by eliciting the dominant evaluative (emotional) components of mediating reactions which feed cues back to the perceptual system, may produce either facilitation of covaluent integrations (vigilance) or disorganization of integrative mechanisms (defense)." He further suggests that this process may be mediated by lower brain centers.

Conceiving of discrimination at levels below awareness as only a rather gross type of discrimination has a certain plausibility to it. It avoids the concept of the "unconscious" as a sort of supernatural entity with mysterious powers. On cursory examination it would even appear to have experimental support. Binder (4), among others, has pointed out that one or more of the characteristics of a stimulus may be discriminated in the absence of sufficient discrimination of the remaining characteristics to permit specific identification. Thus at a brief exposure we may be able to discriminate the color of a stimulus without perceiving its texture or its shape. And if we can discriminate the color of a stimulus without also perceiving the texture and shape necessary for specific identification, why couldn't we be expected to discriminate the affective value of a stimulus in the absence of a specific identification?

The difficulty with such a conclusion is this. Generally the meaning of a stimulus is conveyed, not by one or two physical characteristics of the stimulus, but by the total aggregate of characteristics. In the case of words which are frequently used as stimuli in perceptual defense studies, the affective meaning is conveyed only by the perception of the complete word, not by isolated characteristics such as word length or a tall letter at the beginning of the word. For discrimination of affective meaning or of threatening stimuli to occur, some place in the nervous system the complete word would have to be identified. Thus we find that what at first seems to

be an assumption of only very gross discrimination at levels below awareness is in fact an assumption of very precise discrimination, provided it is to be used to explain the perceptual defense data.

We can illustrate this point by considering the well-known experiment on subception of Lazarus and McCleary (37). They conditioned the GSR to five nonsense syllables and not to another five, and found the tachistoscopic exposure of the nonsense syllables resulted in larger average GSRs to the shock syllables even in the absence of correct verbal identification of the specific nonsense syllable exposed. Their results can not be explained by assuming a gross discrimination capacity at the level of subception. An interpretation of the data in terms of perception at levels below awareness requires that the stimulus words be very precisely identified at the subceptual level unless there was some artifact such as the more frequent occurrence of certain letters among the shock words than among the nonshock words. In their experiment this did not seem to be the case. Shock words could be distinguished from nonshock words only if all of the letters in their proper sequential order were perceived.

Experimental Studies of Discrimination at Levels below Awareness

What experimental evidence is there of the capacity for discrimination at levels below that available to verbal report? One can distinguish three basic methodological approaches to this problem. The first consists of attempting to condition some "nonvoluntary" response, such as pupillary contractions or GSR, to a stimulus presented with an intensity that has been previously determined to be subliminal. The second approach is to condition a GSR or similar nonvoluntary response to a stimulus at supraliminal levels and then to assess the threshold for this stimulus via both the GSR and the verbal response. The third methodology, which is some-

times combined with the other two, is to condition a GSR or some other nonverbal response (or assume prior conditioning of this response) to a stimulus that is then presented at around threshold values. The nonverbal response is then observed to determine whether it shows greater than chance relationship with the stimulus on those trials when the verbal judgment was incorrect. I shall not attempt to summarize here all the different studies using these methodologies, but instead select representative experiments which illustrate the general findings and the methodological problems encountered that limit the interpretation of the data.

Subliminal Conditioning

The evidence for subliminal conditioning is for the most part negative or at best controversial. The more carefully controlled studies have offered but little evidence that subliminal conditioning is possible. One of the more intriguing studies is that of Baker (3), who reported that the pupillary light reflex could be conditioned to a tone below the absolute threshold. Using the below-threshold tone as a conditioned stimulus (CS) and change in illumination as the unconditioned stimulus (US), he was successful in establishing a conditioned response to the subthreshold tone within three trials. In addition, responses conditioned to the subthreshold sound generalized less than responses conditioned to suprathreshold values. They were also more stable and resistant to extinction.

In a repetition of the study, Metzner and Baker (39) obtained essentially the same results, but a number of other investigators have been unsuccessful in duplicating this experiment (30, 59). Wedell, Taylor, and Skolnick (59) in a repetition of the Baker experiment found no evidence of conditioning in any of the subjects they employed. In fact a number of careful investigators have been unable to obtain pupillary conditioning even to suprathreshold stimulus values (29, 53, 54, 55).

In a recent experiment Taylor (56) claims to have demonstrated subthreshold conditioning, but here again

this interpretation of his results is open to serious question. He presented six different geometric figures tachistoscopically to subjects one at a time to determine a level of exposure where the subjects were unable to identify at better than chance level which specific geometric figure was being presented. He was successful in obtaining GSR conditioning at the tachistoscopic exposure level where the subjects were unable to identify the different geometric figures correctly.

While Taylor interprets his data in terms of subthreshold discrimination, there is a simpler and more plausible explanation of his findings. The subject's being unable to identify which geometric form was being tachistoscopically exposed is in itself insufficient evidence for assuming that all six geometric forms at this level of exposure resulted in the same perceptual experience. It is more than likely that different perceptual experiences were associated with each of the six geometric forms at the level of exposure selected by Taylor. This is not to say that the information the subject received was sufficient to specify what form was being shown, but merely that different experiences or perceptions were associated with the different forms. For example, the exposure of a square might have led to a description on the part of the subject of a perception of a dark smudge that seemed to sort of expand in the field. A free report of his perception following exposure of the triangle might have led to some description such as, "I had the feeling of sort of a slanted line moving across the field." While these perceptions would not be sufficient to enable the subject to identify which figure was being presented, they none the less are distinguishable perceptions and therefore capable of differential conditioning or even differential verbal response, provided proper feedback is given.

In Taylor's experiment a necessary control would have been to determine whether the subjects reported the tachistoscopic exposure of a square and a triangle as identical perceptual experiences or as different. By asking the subject to say "same" or "different" following

successive exposures of geometric figures, one could have determined whether differential perceptual experiences existed at the verbally distinguishable level.

The possibility of subliminal conditioning of finger retraction was thoroughly and carefully explored by Newhall and Sears (41). Using electric shock as the US and a light as the CS, these investigators were successful in obtaining conditioned finger retraction to light intensities at the absolute threshold. They determined the absolute threshold by the method of constant stimuli using the 50% point as the threshold. Threshold determinations were also obtained throughout the experiment. Some of their subjects were found to give conditioned finger retractions to light intensities below the absolute threshold. (Four of their seven subjects gave a total of five conditioned finger retractions in the sigma intervals of -.5 to -1.0.)

Newhall and Sears are properly cautious in interpreting their data in terms of subliminal conditioning. They pointed out that while some conditioned finger retractions were obtained at values below the absolute threshold, the threshold in their study was defined as the 50% point. Thus by definition of the threshold, the subjects verbally would report on a number of occasions having seen the light at values that actually fall below the arbitrarily defined threshold. In other words, the subject is responding correctly better than chance in his verbal report at values below the statistically defined threshold.

Further confirmation that there was no superdiscrimination by unaware processes in their experiment is found when the conditioned response is itself used to estimate the absolute threshold for light. By using the later sessions in the experiment when successful conditioning had been well established in most subjects, it was possible to estimate the thresholds from the frequency of conditioned finger retractions. It is interesting that the absolute threshold computed from the frequency of conditioned

responses corresponds almost exactly to the value obtained by using the verbal judgments.

The most recent and perhaps the most thorough investigation of subliminal conditioning appears to be that of Wilcott (60). He attempted to condition GSR to subthreshold auditory intensities at several different frequency levels. His subjects were given preliminary training on threshold determination and the stimulus intensity selected for subthreshold conditioning was the highest intensity that the subject failed to report hearing during the threshold determination sessions. While he was successful in GSR conditioning to suprathreshold intensities, no evidence was found for conditioned GSRs in response to the subthreshold intensities at any frequency level. Interestingly enough, Wilcott's experiment appears to be the only one using a subliminal CS that was at a low enough intensity so as not to yield better than chance verbal reports.

From this brief survey of the literature we can see that the evidence for discrimination at levels below those available to awareness is not very impressive. In the cases where studies have obtained positive results, other investigators have been unable to duplicate the findings. In the cases where slight evidence for subthreshold conditioning has been obtained, the results seem to be more readily accountable by other explanations.

There are several sources of artifact that need to be carefully considered before interpreting the data of an experiment in terms of conditioning or discrimination at levels below those available to awareness. Of major importance is the definition of a threshold. It must be born in mind that our definitions of thresholds are arbitrary statistical definitions. In the typical psychophysical method a subject is asked to report "yes" or "no" concerning the presence or absence of the stimulus. Commonly we accept the 50% accuracy level as the threshold. It is to be noted, however, that this procedure insures that the subject is discriminating better than chance at

stimulus levels below our arbitrary threshold designation. If we include a blank stimulus interval in our threshold determination, we can observe the subject's false-alarm rate or his tendency to say "yes" in the absence of any stimulation. Typically in a good subject this false-alarm rate should be 10% or less. Thus any deviation from this 10% point in terms of positive answers or reports that he perceives the stimulus indicates he is now beginning to perceive the stimulus at a better than chance level. At a stimulus intensity that the subject reports perceiving 25% of the time, we are below his arbitrarily defined threshold value but at the same time we are 15% at least above his chance performance level. To demonstrate that conditioned responses can be obtained 15% of the time at this stimulus intensity level is hardly evidence for concluding discrimination below verbal awareness.

A second source of error in interpretation of subliminal conditioning studies revolves around the method used in obtaining thresholds. Here we are particularly concerned with whether or not a forced choice type of threshold measurement is employed. Blackwell (5) has amply demonstrated that by the use of forced choice technique, appreciably lower thresholds are obtained than when the subject has the freedom of reporting "yes" or "no" for each stimulus interval. As will be seen later, failure to use a forced choice technique confounds the threshold with the subject's "subjective confidence."

Assessing Thresholds by Conditioned Responses

A second method for investigating the question of a more sensitive discrimination that occurs at levels below awareness is to condition some response other than verbal to a stimulus and then observe whether this response generalizes to values of the stimulus at threshold intensities. One could condition a GSR to a suprathreshold stimulus and then observe the frequency of conditioned responses as the stimulus was varied in intensity around the threshold level. The frequency of conditioned responses as a function of the intensity of the stimulus

could be used to compute a threshold that could then be compared with the threshold obtained by verbal report. The experiment by Newhall and Sears (41) described above provided an example of this method of threshold determination. From the frequency of conditioned finger retractions they computed a threshold which, as we noted, was identical to that obtained by verbal report.

One of the more interesting experiments of this type is one reported by Davis (12). He made use, not of a conditioned response, but of the spontaneous increase in muscle tension that occurs to a noise. He measured the muscle action potentials in several different muscle groups in response to a sound presented above and below threshold. The data indicate that reliable increases in muscle tension occurred to stimulus intensities that the subjects did not report hearing. Unfortunately, these data can be taken only as suggestive since the method of threshold determination followed by Davis was not very rigorous. The subjects were not given preliminary training on threshold determination nor were they alerted for the intervals in which the stimulus might appear. In addition the instructions given the subjects were not of the type that generally yield a most sensitive threshold.

Recently Dulany and myself have completed an experiment on threshold determination via the GSR which yielded negative results as well as illustrating the futility of attempts to obtain lower thresholds via the GSR as opposed to verbal report. The difference threshold for brightness was determined by the method of constant stimuli. Subjects viewed a dim homogeneously illuminated test field upon which was presented a rectangular test patch of light of controlled intensity. An auditory signal designated the intervals when stimuli occurred. In order to correct for guessing, blank test trials were randomly interspersed with the test patch. Subjects reported "yes" if they saw the light and "no" if they did not. After ten days of training during which each subject made 240 judgments on each day, the subjects were given conditioning sessions. A GSR was conditioned to a suprathreshold intensity of the

test patch with electro-shock to the ankle as the US. A partial reinforcement schedule was used in establishing the conditioned response. Following conditioning, three more daily sessions were employed to establish the GSR threshol as well as a concurrent verbal discrimination threshold. To make the thresholds obtainable from the GSR and the verbal response directly comparable, a modification of Blackwell's forced choice technique was used. For presenting the test patch, two intervals of three seconds' duration each occurred, separated by a three-second interval. Subjects were required to report which interval the light or test patch occurred in. GSRs were recorded continuously during the threshold determination. If the GSR to the first interval was greater than the GSR to the second, this was considered the GSR's choice on that particular trial. In order to maintain stable conditioned responses, reinforced presentations of suprathreshold intensity were randomly distributed among the test trials.

In Figure 1 the thresholds are plotted in per cent of correct responses as a function of stimulus intensity for the GSR responses and the forced choice verbal responses. Probably the most meaningful comparison is in terms of the inflection points where the curves begin to ascend from the chance level. These are designated in the graph. As can be seen, the lowest threshold is obtained by forced choice verbal responses. The GSR threshold is appreciably higher

It is to be noted from Figure 1 that even when the subjects were 100% correct on their verbal judgments, the GSR responses never achieved a higher level of accuracy than 85%. This failure of the GSR to be as accurate on suprathreshold stimuli as the verbal judgment raises the serious question as to whether one could expect the GSR to show greater sensitivity in the critical region where responses are just beginning to deviate from chance. The GSR is a very sensitive response and is influenced by a number of uncontrolled extraneous variables. Thus it seems unlikely that a response that has less accuracy and precision than a verbal response at suprathreshold values could be expected to yield a more sensitive accurate measure

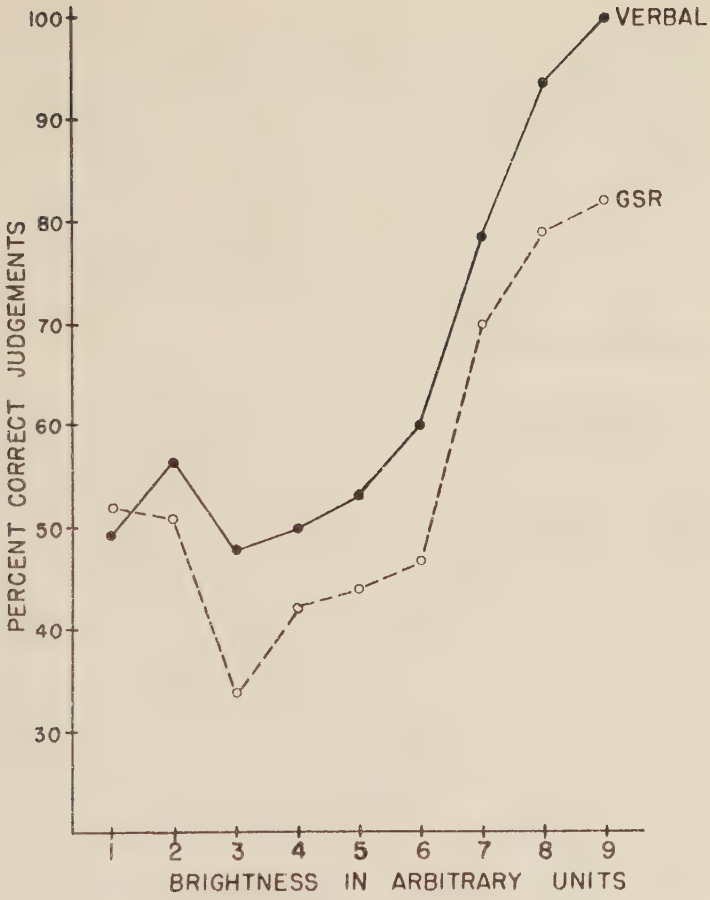

FIG. I.

at lower stimulus intensities.

So far in our examination of evidence for discrimination capacities that lie below the levels available to verbal report, we have been unsuccessful in finding any unequivocal evidence for the existence of such capacities. Davis' (12) results are perhaps the most suggestive but replication with more careful controls and attention to verbal threshold measurement is necessary before this evidence can be accepted. But even if replication should

substantiate Davis' finding that muscle tension varies at levels of stimulation below those capable of differential verbal report, there is still the question of what meaning this has for behavior. For subliminal responses of some part of the body to external stimulation to be important psychologically, it needs to be shown that these subliminal responses are capable of serving as cues for differential responses and are not just isolated neural action potentials in the sensory tracts or muscle twitches.

The Subception Effect

There is a third class of experiments that are erroneously but none the less often considered to reveal discrimination at levels below that available to awareness. These experiments use the same experimental arrangement as that employed in the previous approaches. A nonvoluntary nonverbal response is conditioned to some suprathreshold stimulus or stimuli and then stimuli are presented at about threshold value. The subjects are required to give a verbal response, and the incidence of the concurrent nonvoluntary conditioned responses is noted. However, in these experiments the conditioned response is not used to compute a threshold. Instead the data are analyzed in terms of whether or not a conditioned response occurs more often than chance would indicate in the absence of a correct verbal response. Lazarus and McCleary (37) in their well-known subception experiment were among the first to use this type of analysis as evidence for discrimination without awareness.[2]

Lazarus and McCleary conditioned GSRs to five of ten nonsense syllables and then exposed the nonsense syllables individually in a tachistoscope at a duration level where verbal recognition was about 50% correct. They found that the average GSR to shock syllables was

[2]It is to be noted that Newhall and Sears (41) also briefly considered this type of analysis and pointed out an explanation of this subception effect that is essentially the same as that which I elaborate below.

greater than to nonshock syllables on those trials where the subjects were unable to give the correct verbal identification of the syllable. They interpreted their data in terms of subception--"a process by which some kind of discrimination is made when the subject is unable to make a correct conscious discrimination."

While nearly all the discrimination without awareness experiments that we have analyzed so far would have lent their data to this type of comparison, the novelty of the Lazarus and McCleary approach was the introduction of this method of analysis. It is not the same as determining a differential threshold for the GSR and the verbal response. Lazarus and McCleary did not compute separate thresholds. While they report that the tachistoscopic level was set so as to obtain approximately 50% correct verbal recognitions, they do not report the differential accuracy of the GSR on classifying shock and nonshock nonsense syllables. However, an estimate of the comparative accuracy of the verbal report and the GSR on differentiating between shock and nonshock nonsense syllables in their experiment can be estimated from an unpublished replication of their study that I have carried out. This replication was successful in duplicating the subception effect. However, a comparison of the relative accuracy of the GSR and the verbal report shows that if an observer wished to predict whether a shock or a nonshock nonsense syllable had been presented the subject and could use only one of the subject's responses, the verbal report or the GSR, he would have been considerably more accurate using the subject's verbal report.

At the tachistoscopic exposure level used in the replication, approximately 45% of the verbal reports correctly identified the specific nonsense syllable. The most optimum weighting of GSR values gave approximately 60% correct classifications into shock or nonshock categories. The chance expectancy for correct nonsense syllable identification in a verbal response is 10%, whereas for the dichotomous classification of shock vs. nonshock

by the GSR, chance level is 50%. An observer attempting to determine whether the subject had seen a shock or a nonshock syllable would have been correct in his designations 60% of the time using the GSR but using the verbal response he would have been correct 45% of the time plus one-half of 55% or a total of 72.5% of the time. This is based upon the assumption that of the 55% of the time the subject's verbal response was incorrect as to the specific nonsense syllable exposed, half of the time he would have given a nonsense syllable that was a shock syllable and the other half of the time a nonshock syllable.

While the Lazarus and McCleary experiment does not demonstrate that the threshold of a GSR response is lower than the threshold for verbal identification, none the less, their finding that in the presence of an error in the verbal response system the GSR response has better than chance accuracy has some important implications for our concept of behavior without awareness. Elsewhere (20) I have analyzed the subception experiment of Lazarus and McCleary in terms of a partial correlation model and pointed out that in their experiment the possibility existed that the results were due to an artifact resulting from an artificial restriction on the number of verbal responses the subject could use. Since subsequent experiments carried out in our laboratory have indicated that verbal response restriction is not sufficient to account for the subception effect, it is necessary to review briefly the partial correlation interpretation in order to understand the results of these experiments and the implications they have for behavior without awareness.

Essentially the subception experiment consisted of having the subject give two concurrent responses to a stimulus, namely, a verbal response and a GSR. The obtained subception effect can be expressed as a partial correlation between the stimulus and the GSR when the verbal response is held constant. Conceived in this way it is possible to specify the general circumstances under which such partial correlations or the subception effect

would be obtained.

The necessary and sufficient conditions are fulfilled when both responses are independently correlated with the stimulus, but less than perfectly, and less than perfectly correlated with each other. The independent correlation between the two responses to the stimulus means that both the verbal response and the GSR are responses to the stimulus as perceived rather than the GSR being a response to the verbal classification of the stimulus. Less than a perfect correlation between the verbal response and the GSR can be alternatively stated as saying that these two response systems have non-correlated error terms. Thus, variables which may affect the accuracy of use of verbal responses may not concurrently affect the accuracy of the GSR and vice versa.

All the above conditions were met in the Lazarus and McCleary experiment. The tachistoscopic duration of stimulus exposures was arranged so the verbal report of the subject was only about 50% accurate, or so that the correlation of the verbal response with the stimulus was considerably less than perfect. The GSR in their experiment was also less than perfectly correlated with the stimulus. Since these investigators actually obtained a significant partial correlation between the stimulus and the GSR, we know that the remaining conditions, namely, less than perfect correlation between the two responses and an independent correlation of each response with the stimulus, existed.

Formulation of the results of the subception experiment in terms of partial correlation enables us to state specifically just what the experiment demonstrated. It succeeded in showing that both the verbal response and the GSR are each, at least to a degree, independently correlated with the stimulus and are partially independent of each other. In other words, the GSR is, at least partly, determined by the stimulus and the intervening perceptual process and not solely by the verbal response. This re-

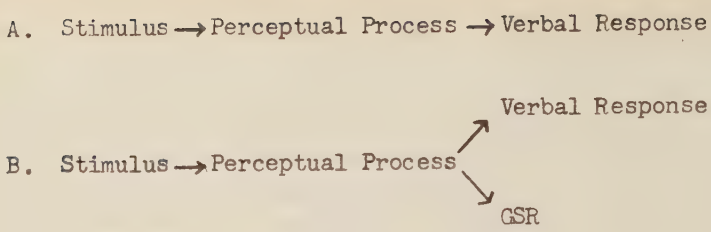

Figure 2

lation can best be seen in terms of the diagrams in Figure 2. If the relation was as depicted in A, then it would have been impossible to have obtained a subception effect. The finding of a subception effect requires a process of at least partial parallel response systems as diagramed in B.

We have carried out a series of experiments in our laboratory in order to test this partial correlation formulation and to see what implications it had for the concept of awareness. In the first study (18) we investigated the concurrent GSRs and verbal responses to a series of different sized squares. While the Lazarus and McCleary experiment dealt with a fairly complex perceptual situation, the formulation of their experiment in terms of the partial correlation model suggested that the subception effect should be obtained in a simple stimulus generalization task.

One group of subjects had a GSR and a verbal response (the number "6") conditioned to a 35mm. square stimulus with electric shock used as the US for the GSR. After conditioning, these subjects were presented with a series of 11 squares of different sizes--the conditioned square along with five larger and five smaller squares. The squares were presented one at a time under conditions of absolute judgment with the instructions to call the smallest square "1," the largest square "11" and to assign the intermediate numbers to the intermediate squares in order of increasing size. They were informed that the conditioned square, which they had been trained to call "6,"

was actually the mid-square of the series and occupied position 6.

This particular experimental task was chosen since it presented a simplified situation for observing the subception effect. It fulfills the conditions specified above under which the subception effect should be obtained. The discriminative problem had been found previously to be beyond the capacity for perfect performance so that the correlation between the stimuli and the verbal responses would be less than perfect. Furthermore, studies of GSR generalization have shown that differential magnitudes of GSR could be expected to the different size stimuli, and here again the correlation of the response with the stimulus would be less than perfect. The remaining condition, the two response systems have a noncorrelated error term, had already been demonstrated by Lazarus and McCleary. With these conditions met, it was expected that the GSRs of the present study would tend to vary systematically in magnitude with the stimulus when the verbal responses were held constant.

To investigate one source of noncorrelated error between the GSR and the verbal responses, a second group of subjects was used. These subjects received identical treatment with those of the first group during the training phase of the experiment. Prior to beginning the test for generalization, however, the subjects in the second group were instructed somewhat differently--to respond with "6" when the training square occurred and to say "no" if the square shown was larger or smaller than the training square. We designated this group as the 2-response group, and first group as the 11-response group.

The results of this study confirmed our expectations. Not only did both the 2- and the 11-response groups show characteristic generalization gradients of GSR as a function of stimulus size, but there was no difference in the GSR generalization gradients between them. Maximum GSR deflections occurred with the mid-sized or condi-

tioned square and progressively smaller magnitudes of GSR were elicited by squares smaller or larger than the conditioned square.

While there was no significant difference in the GSR generalization between the 2- and the 11-response groups, there was, however, a significant difference in the verbal response generalization. In keeping with findings of a preliminary experiment the 2-response group gave a greater absolute number of generalized conditioned verbal responses than did the 11-response group. They used the verbal response "6," not only more often to the stimulus number 6, but also more often to squares larger and smaller than the conditioned square. Both groups again showed typical generalization gradients but the gradient for the 2-response group was consistently above the gradient obtained for the 11-response group.

These results show that the number of available verbal responses is a significant determiner of verbal response generalization but has no effect upon the generalization of the GSR. We have, therefore, suggestive evidence of a variable that would differentially affect verbal and autonomic response systems.

To determine whether a subceptionlike effect was obtained in our experiment, the GSR deflections were analyzed for all trials on which subjects had given as a verbal response the number "6." The average GSR value as a function of the real stimulus size was then determined and again generalization gradients were found. For example, if, as frequently happened, a subject called square 4 in the series by the verbal response "6," the GSR accompanying this response, on the average, was less than when the "6" response was given to the number 6 square. In othe words, when the verbal response was held constant, i.e., all "6" responses, the GSR was still found to vary systematically with the actual magnitude of the stimulus.

Evidence for the subception effect was also found if we examined the GSRs accompanying all verbal responses

that were other than the response "6." Again it was found that when the verbal responses were controlled in this manner, the GSR varied systematically with the actual stimulus magnitudes, i.e., when an individual in the 2-response group gave a "no" response to stimulus number 4, the accompanying GSR averaged less under these circumstances than when a "no" was given to the number 6 square.

These results confirmed our prediction that a subceptionlike effect could be obtained on a relatively simple discriminative task. Viewed from the standpoint of our partial correlation model, the subception effect is not surprising, but is actually the expected outcome. The real experimental problem would appear to lie in the variables that determine the noncorrelated errors between two concurrent responses.

Although we confirmed the subception effect in this experiment we cannot conclude discrimination without verbal awareness. As in the Lazarus and McCleary experiment, we have not made sure that artificial verbal response limitations were not responsible for the effect. How such an artificial limitation could have operated can be illustrated in this manner. Sometimes a subject giving the response "5" to a square may have been uncertain whether the response should be a "5" or a "6." Other times in giving the same response "5" he may have been uncertain as to whether the response should be a "5" or a "4." The subject may well have been aware of his uncertainty and could have communicated it verbally to the experimenter if permitted to use responses other than the number 1 through 11. We can assume further that the direction of the uncertainty, whether, for example, between "5" and "6" or between "5" and "4," is probably associated with the actual magnitude of the stimulus. That is, when the subject was uncertain as to whether "5" or "6" was the correct response, most often the square was a 5 or larger. On the other hand, if his uncertainty was between "5" or "4," the correct size would generally be "5" or smaller.

But under the experimental conditions, the subject was not permitted to tell about the direction of his uncertainty. Instead he was required to give a single response. The experimental conditions do not allow him to state verbally all the discrimination he is capable of making.

The GSR, not being discrete and not being limited to a single category of response as is the verbal behavior, could make compromise reactions under the above circumstances. For example, the typical or average GSR to stimulus number 5 might lie between the units of 60 and 70 microhms. When the subject responded "5" but was uncertain as to whether the response was a "5" or a "6," we would expect the GSR to have a value toward the higher end of this range. When the uncertainty is between a "5" and a "4," on the other hand, the GSR would have a value toward the lower end of this range. Thus we have a situation where the GSR is able to convey information additional to that given by the verbal response. But we cannot conclude that we have found discrimination at the autonomic level without verbal awareness. It is necessary that we carry out a further control condition in which we allow the subject a more refined verbal response system: one that will allow him to communicate his direction of uncertainty.

A second experiment (17) was performed in order to determine whether the subception effect would disappear when the subjects were allowed a wider range of verbal responses. For this study an absolute judgment task was used consisting of the judgment of 11 different sized squares. The subjects were allowed to use the numbers 1 through 11 to indicate their judgment of square size. But in addition to this verbal judgment, subjects were permitted a second guess following their first judgment of a square. The use of the second guess was designed to give information concerning direction of the subject's uncertainty in his judgments.

Instead of using an autonomic response such as the GSR for a third response, we made use of lever positioning.

Concurrently with his judgment of a square, the subject moved a lever a distance proportional to the perceived size of the square. This substitution of a lever movement response in place of an autonomic measure was made from several considerations. First, the lever movement had some of the same characteristics that the GSR response had had in our previous study and in the experiment of Lazarus and McCleary. That is, it was a continuous response as opposed to a discrete categorical response. In view of this continuous characteristic of the lever response, it could be expected from our partial correlation interpretation of subception that similar effects would be obtained with this response as were obtained with the GSR in the other study. A lever response such as this also offered advantages of much greater control and freedom from error.

In analyzing the data, the highest average correlation between stimuli and responses was that between the stimulus and the first verbal judgment, although the correlation between the stimulus and the lever response was not significantly nor appreciably lower. All first order partial correlations between responses and stimuli were moderately high and statistically quite significant, i.e., the partial correlations between the stimulus and the verbal response with the second guess held constant, the correlation between the stimulus and the first verbal response with the lever response held constant, the correlation of the lever response and the stimulus with the verbal response held constant, etc.

Turning to the second order partial correlations where we determine the relationship between the stimulus and each response system with the remaining two response systems held constant, we again found all the partials to be significant. When multiple correlation coefficients were computed for each subject, using the various combinations of the three different responses given to a stimulus, it was found that a multiple correlation using all three responses to predict the stimulus accounted for significantly greater variance than the combination of any

two responses as predictors.

While these results suggest a number of conclusions, the one that is most germane to our present discussion is this. Even when the subject is allowed verbal responses to reflect the direction of his uncertainty, there is still an additional amount of discrimination that is carried by the nonverbal response. In keeping with the definition of awareness in terms of verbalization, this result would certainly suggest the human organism is capable of making discriminations beyond awareness. However, it would seem more meaningful to think of this result in terms of limitations of our language system to reflect perceptions.

In several follow-up experiments in this area we not only have succeeded in replicating the results reported above but have also found that the precision of the verbal reports or judgments has little or no effect upon the precision of the nonverbal judgments. Also greatly increasing the number of verbal labels available for the first judgment was found to have little or no effect upon the additional discrimination that is carried by the second guess and the nonverbal response.

When the results of this series of experiments are taken together, several conclusions become evident. First, we can see that the results obtained by Lazarus and McCleary in their subception experiment are not likely a result of artifactual restriction upon the number of verbal responses allowed their subjects. Rather the subception effect seems to be inherent in any experimental arrangement where subjects are allowed two or more different responses to a single stimulus, provided each of the responses is less than perfectly correlated with the stimulus and less than perfectly correlated with the other responses. We need to conceive of the perception as some internal process. The responses by which we infer perception irrespective of whether they are verbal, nonverbal motor responses, autonomic responses, or other nonvoluntary responses, need to be conceived as independent or largely independent responses to this internal per-

ception. Such a view of perception has been presented by Garner, Hake, and Eriksen (26) along with a discussion of converging operations by which perception can be separated from the responses from which it is inferred.

A second conclusion that can be drawn from the above-described experiments pertains to the limitation of language in reflecting the discrimination the organism is capable of making. We have seen that increasing the number of separate verbal labels does not increase or eliminate the surplus discrimination the organism can make by other responses. Both a second-guess verbal response and a simple motor response give information in addition to what is provided by a single verbal response. This conclusion has rather important bearings upon any definition of awareness that equates it with verbal responses.

Closely related to this conclusion is a third that questions the importance that has generally been attributed to verbal labels in discrimination. From the results of the above experiments it is apparent that considerable discrimination can occur in the absence of specific verbal labels. The finding that a simple motor response is largely independent of the verbal response in discrimination tasks and, in addition, seems to be unaffected by the precision of the verbal labels, would cast considerable doubt upon assumptions concerning the value of verbal labels in the precision of discrimination. This conclusion has importance to the problem of unconscious processes primarily in terms of the distinctions that have been drawn by Dollard and Miller (14). Their distinctions between conscious and unconscious processes hinge critically upon the assumption that language or verbal labels provide increased precision in discrimination as well as providing cues that in turn can be used to mediate transfer and reduce overgeneralization. Evidence that verbal labels are not as critically important in these functions would necessitate modifications of their distinction between conscious and unconscious functions.

Finally it is important to note that the above series of studies show that the subception effect originally elaborated by Lazarus and McCleary does not need to depend upon some involuntary response such as the GSR. It can be more reliably obtained through using voluntary responses that have less random variable error. The effect even appears in situations where both of the concurrent responses to the stimuli are verbal. These findings would seem to mitigate quite effectively against any interpretation of the subception effect in terms of the capacity of the organism to discriminate at levels below those available to verbal report, i.e., awareness.

There is one further experiment I would like to report in connection with the problem of concurrent responses to the same stimulus because it tends to summarize the implications that have been brought out by the above-mentioned studies as well as to deal with a problem that Osgood (42) has recently advanced.

As we have seen earlier, Osgood has suggested that the meaning or affective evaluation of a stimulus may occur prior to specific recognition in the perceptual process. As a means of testing this possibility we had a group of subjects rate a pre-selected list of 60 words on a seven-point scale for pleasantness and unpleasantness. Ratings of 1 were used to denote the words that seemed to arouse in the subject the most pleasant feelings whereas 7 was used for the words giving rise to the most unpleasant feelings. Approximately two weeks later, individual subjects were presented tachistoscopically the ten words to which they had given the most pleasant ratings and the ten words to which they had given the most unpleasant ratings. All 20 words were exposed randomly at a level of illumination well below recognition threshold. After the exposure of each word the subject was asked to make a judgment of whether it was pleasant or unpleasant as well as to guess what the specific word was. After the 20 words had been presented at the initial illumination level, the illumination was increased slightly and the words were presented in a new random order. This procedure

was continued until an illumination level was reached where 90% of the words had been specifically identified.

The subject's pleasant-unpleasant judgments of a word prior to correct recognition were compared with his ratings of the words by means of a four-fold Chi-square table. For the 17 subjects, results were significant beyond the .001 level of confidence. This indicates that the subjects were considerably better than chance in verbal classification of the emotional tone of the stimulus at the tachistoscopic duration levels where they were unable to specifically identify the stimulus. A product-moment correlation of .48 was obtained between the average ratings of pleasantness of a word and the proportion of pleasant judgments that were received prior to correct recognition. A careful examination for artifacts such as differences of word length between pleasant and unpleasant words or combinations of letters that occur more frequently in the pleasant as opposed to the unpleasant failed to reveal any characteristics of this nature that would have enabled the subject to have correctly identified the pleasant or unpleasant nature of the word on the basis of a partial cue.

One would be tempted to interpret these results as indicating that, indeed, the nervous system makes an affective judgment or classification of stimuli at levels before specific identification can occur. However, there is a simpler explanation for the results, one that fits more neatly some of the supplementary findings. Again we can evoke the explanation of partial correlation between two concurrent responses to the same stimulus. In terms of the partial correlation model we will assume that the exposure of the stimulus word gives rise to a perceptual process which turn activates responses in both of the response systems available to subjects, the specific word responses and the pleasant-unpleasant judgments. Since the exposure of the stimulus is deliberately made short, the perceptual process is weak and there is considerable error in both response systems. If the sources producing error in the two response systems are not perfectly correlated and both systems are operating above chance accuracy, then

we would expect the affective judgments to be correct better than chance on those trials when the specific word responses were incorrect. Also the reverse would hold.

As an example, let's assume that both the specific responses and the affective judgments are operating at 20% above chance level and, for ease of exposition, let us assume that the two response systems are completely independent of each other. Out of a hundred trials the specific identifications would be correct 20 times and wrong 80. But in keeping with our assumptions of independence the affective judgments would not also be wrong on these 80 trials; nor would they yield a chance level accuracy (40-40). Instead, since the affective judgments are also operating at 20% better than chance level (70%) we would expect the affective judgment to be correct on 56 of these 90 trials.

In this example we have assumed complete independence between the two response systems which probably never actually occurs. However, the effect elucidated in the above example is only attenuated, not eliminated, as the correlation between the two response systems increases. Only in the case of perfect correlation or agreement between the responses in the two systems will this effect cease to operate.

There is an additional finding from the data of this experiment that supports the interpretation in terms of partially independent responses and argues against interpretation in terms of an affective evaluation that occurs in lower centers of the nervous system. The data show that if an independent observer wished to infer whether the subject had been exposed to a pleasant or unpleasant word he would do at least as well using the subject's specific responses as using his judgments of pleasant or unpleasant. When the subject's word guesses are about 50% correct, the affective judgments are about 75% correct. But the accuracy of the word guesses is nearly 50% above chance level, while the affective judgments are only about 25% above. To classify the stimuli as

pleasant or unpleasant using only the subject's responses, we could do as well using his specific word guesses as we could do using his affective judgments. By using the affective judgments we would correctly classify 75% of the words as pleasant or unpleasant but we would be at 75% accuracy on the basis of the word guesses, since in addition to the 50% correct word identifications, we could expect to get half of the incorrect word guesses by chance correctly classified as pleasant or unpleasant.

It would seem that if affective discrimination occurs temporally prior to specific verbal identification or at levels below awareness, then such affective discrimination should have a lower threshold than verbal identification. Although our data show that when the verbal identification is incorrect, the affective judgment is correct more often than can be attributed to chance, we do not find any evidence the affective judgment has a lower threshold than verbal identification. The affective judgments do not begin to show a deviation above chance accuracy at a lower illumination level than do the specific word guesses.

I have devoted considerable attention to the subception effect, first, because the experiment of Lazarus and McCleary generated considerable interest, particularly in connection with perceptual defense phenomena, and, secondly, because the experiment has been frequently and erroneously interpreted as demonstrating discrimination at a level below that available to verbal report. As we have seen, the subception effect is real enough but it has nothing to say or offers no proof concerning the capacity of the GSR to reflect a more sensitive discrimination than that which is available through the subject's verbalizations. Instead the subception effect focuses attention upon the limitations of verbal responses in conveying the individual's perceptual experiences. By formulating the effect in terms of a partial correlation model we have the advantage of being able to direct our attention upon sources of noncorrelated errors between two or more concurrent response systems. We have already indicated

some of the factors that will lead to increase in the non-correlated errors between concurrent responses, and it is interesting to speculate on the extent that variables such as differential reward or punishment might have upon decreasing the correlation between verbal and certain nonverbal responses.

To say that the subception effect does not provide evidence for discrimination at levels below awareness is not to deny that the phenomenon is important to our general concept of awareness. I strongly suspect that a considerable amount of the evidence that the clinician finds for unconscious processes is in the nature of discrepancies between concurrent responses. The clinician may note that the subject's autonomic responses and evidences of emotion do not correspond, i.e., do not correlate very well, with his verbal statements. This lack of correlation is the basis for his inference of unconscious processes or defense mechanisms at work. It is quite possible that anxiety that has been conditioned to words or to thoughts in the patient's past history may tend to maximize the independence between his verbal statements and his nonverbal responses to various stimuli.

Awareness and the Subject's Subjective Confidence

There is one other experimental approach to the problem of awareness that deserves brief consideration. It differs from the other methods in that a discriminated verbal report is not used as the criterion or definition for awareness. Instead awareness is defined in terms of the subject's statement as to whether or not he feels he has perceived the stimulus or whether he feels he is merely guessing. The evidence consists in showing that when the subject expresses zero confidence, i.e., subjectively pure guessing, he none the less responds correctly to the presence or absence of the stimulus at better than a chance leve

Peirce and Jastrow (43) were among the first to report this phenomenon. Working with the difference limen on

weight judgments, Jastrow's data clearly indicate that in the presence of a subjective report of "no confidence," his judgments of heavier or lighter were still correct significantly better than chance. Since then this phenomenon has been a frequent finding in a number of experiments. In fact as Adams (1) points out it is a stable enough phenomenon to permit its use as a classroom exercise. Representative of experiments obtaining results on this aspect of awareness are those of Fullerton and Cattell (25), Miller (40), Coover (11), and Williams (61).

Although these experiments are frequently and uncritically used to support contentions concerning discrimination without awareness where awareness is defined in terms of a discriminated verbal report, it is to be noted that these experiments do not fit this definition. In these experiments a discriminated verbal report is obtained. The evidence for unawareness is in terms of the subject's report of lack of confidence in his verbal judgments. While this subjective confidence in his reports is an interesting and legitimate problem, it has no operational similarity to the type of experiments we have considered above. An equation of awareness with verbalization, by definition, means that any verbal judgment is one made with awareness irrespective of whether the subject has little or no confidence in the judgment.

The major difficulty with experiments of this kind is that they place on the individual subject the responsibility for establishing the criterion of awareness. We have no way of objectively knowing that when subject A says a given judgment was a pure guess and subject B makes a judgment and states it was a pure guess that the two subjects are talking about the same phenomena or that they are using the same criteria for guessing. In experimental work in our laboratory we have found very marked individual differences for the intensity range between where a subject shows better than chance guessing in his responses and the point where he begins to report confidence in his judgments. For some subjects the stimulus intensity where responses are better than chance almost

coincides with his confidence threshold. For other subjects there is a great range between where guesses are above the chance level and the point where confidence is reported in the judgments.

We are presently engaged in several experiments on what we term the subject's "subjective confidence interval." We have defined this as the difference between the threshold determined by a forced choice technique and that obtained by a "yes" or "no" response. Under the forced choice technique two intervals are presented to the subject and he is forced to choose in which interval he thinks the stimulus appeared. In the yes-no technique a single interval is presented each time to the subject and he is required to say "yes" or "no" depending upon whether he perceived the stimulus during the interval. The difference between the thresholds obtained by these two techniques we have termed the "subjective confidence interval" and we have found very marked individual differences in this measure. We are currently investigating several personality correlates of the subjective confidence interval and at present have several suggestive leads in this direction.

While research on this particular aspect of awareness may be fruitful, none the less at our present stage of knowledge we are not justified in including data from these types of experiments with those that have been discussed in the preceding sections. Not only are they operationally dissimilar, but more important are the marked individual differences in subjective confidence interval. As long as we rely upon the subject himself to report whether or not a verbal judgment was made with awareness, we are in fact using as many different criteria of awareness as we have experimental subjects.

Perceptual Defense

The evidence surveyed so far does not lend support to any conception of the "unconscious" as a supersensitive discrimination system. But how then are we to account

for the phenomena of perceptual defense? To deny this phenomenon would seem to fly in the face of a rather weighty amount of experimental evidence. The question would no longer appear to be whether perceptual defense can be experimentally demonstrated but rather how this defense occurs. The problem that has generated the most opposition revolves around the question of how the subject can know what to defend against in his perception. Many would argue that the fact of perceptual defense is itself evidence for a supersensitive discriminating "unconscious."

There are two possibilities to account for the perceptual defense phenomena. The first of these may be that a superdiscriminating unconscious actually exists. In this case the experimental attacks on the problem outside the area of the perceptual defense research have not been sufficiently thorough or extensive to demonstrate the existence of a supersensitive discriminating system that lies below levels of awareness. The other possibility is that the phenomena can be explained without postulating unconscious discrimination.

The evidence we have surveyed so far on unconscious discrimination or discrimination without awareness shows that under the most favorable conditions verbal responses give us the lowest or at least as low a threshold as any other response measure. But note, the experimental situations under which verbal response thresholds are determined are so constructed as to remove biases from the verbal responses. A psychophysicist in setting about to determine the absolute threshold for light would not use an experimental situation where the subject was to press a button directly in front of him to indicate that he did not see the light and to get up and walk across the room to press a button on the wall if he did. Few would expect such an arrangement would yield as low an absolute threshold as would be obtained if the subject had the two buttons directly in front of him.

In this example we have a very obvious case of response biasing but it is also quite likely that such factors

as anxiety conditioned to certain words can produce at least as extensive biases in verbal responses. There is an extensive experimental literature demonstrating that responses which have become conditioned stimuli for anxiety have a decreased probability of occurrence. In the conditions that pertain during a typical perceptual recognition experiment, as well as those that occur in ordinary life, biases among verbal responses due to anxiety or other factors are not eliminated as they are in the psychophysical threshold experiment.

It seems possible to explain the perceptual defense phenomena in terms of such response variables. Postman (46) has previously advocated an interpretation of research in this area in terms of response factors and more recently Browne and myself (19) have presented an account of how differential response strengths can produce the perceptual defense results.

It order to understand how such an interpretation works it is first necessary that we distinguish both operationally and conceptually between perception and the responses from which it is inferred. Recently Garner, Hake, and Eriksen (26) have presented the logical and empirical basis for such a distinction between response variables and perception, and the evidence summarized above on the subception effect would seem to demand such a distinction.

Figure 3 diagrams perception as a process intervening between stimulus and response. Only the stimuli and re-

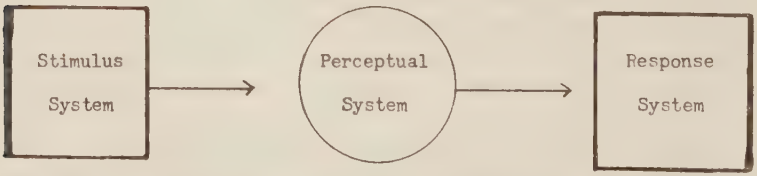

Fig. 3. A schematic of the perceptual problem. The three systems operate in a causal relationship as indicated, although there may be interdependencies. Each system, however, may have independent properties. The properties of the stimulus and response systems can be directly observed; those of the perceptual system must be inferred.

sponses can be observed and therefore perception can be known only as a concept whose properties are objectively determined from the relations between stimuli and responses. However, it is to be noted that such a model does not in any sense imply that perception is identical to the responses from which it is inferred. Once we clarify our thinking by explicity recognizing that perception and response are not identical, then it becomes possible to account for the perceptual defense in the following way.

The perceptual defense phenomenon is obtainable only in the narrow range of values around the recognition threshold or in situations where the stimulus is ambiguous. Under these circumstances the perceptual process itself is weak or incomplete, as can be determined by allowing the subject the freedom of the English language to describe his perception. This weak perceptual process, however, may be assumed to activate various response tendencies. If certain of these responses, or in the case of words, the words themselves, have in the past been conditioned stimuli for anxiety arousal, then it is to be expected that these responses will be less apt to occur relative to other nonanxiety responses that are activated by the perceptual process. The perception has to become much more definite or less ambiguous before the anxiety responses will occur. In this case the perceptual defense phenomenon does not require the assumption of an unconscious manikin who screens the unconscious perceptions and determines what the conscious mind will perceive. It depends upon nothing more mysterious than the empirically derived laws of the effect of punishment or anxiety upon behavior.

Nearly all of the better controlled perceptual defense studies lend themselves to an interpretation along the lines suggested above. Only one study has contained the necessary converging operations that are necessary to rule out a possible response interpretation of the defense effect. Blum (6) compared the frequency with which Blacky Pictures, assumed to be anxiety-provoking, were given as responses following tachistoscopic exposures where the anxiety pictures,

although unknown to the subject, were not exposed. The response frequencies obtained were then compared with the frequencies obtained to the condition where the anxiety pictures were actually presented. It is to be noted that this arrangement provided a control for differential response strength in terms of the picture names or subject matter. If the hypothesis of unconscious perception of the threatening pictures was valid, then it would be expected that this unconscious perception would tend to activate and strengthen defenses over the situation where the pictures were not present. Blum's results show that the anxiety pictures were given as responses significantly less often when they had actually been exposed than when the exposure contained only neutral pictures. Unfortunately this aspect of Blum's findings has not been capable of replication by investigators at other laboratories (47, 51). However, if reproducible results could be obtained showing that anxiety-provoking responses occurred more frequently following the tachistoscopic exposure of the neutral stimulus than following an exposure of the anxiety-provoking stimulus, this would constitute rather powerful evidence for some process of discrimination without awareness.

Blum (7) has also been quite ingenious in adapting perceptual defense phenomena to formulations in terms of psychoanalytic theory. He has assumed that at very low levels of awareness (which he equates with very brief tachistoscopic exposures) the unconscious ought to be more sensitive to the anxiety or threatening stimulus, but as higher levels of awareness are reached (which is equated with longer tachistoscopic exposure) the defensive mechanisms ought to come into play and thus raise the recognition thresholds for the stimuli. Blum has demonstrated this phenomenon by having the subjects report which of several alternative stimuli exposed simultaneously in the tachistoscope appeared "to stand out most." His results show that the anxiety-provoking stimulus tends to be more frequently reported as the clearest although specific recognition of the stimulus is not achieved. At the longer tachistoscopic exposures Blum's data also

show that the anxiety-provoking stimulus tends to be specifically recognized later than the neutral material. As indicated above, investigators in other universities have been unsuccessful in duplicating this defense phenomenon of Blum's, although the vigilance aspect has been successfully replicated.

Some have interpreted the finding of vigilance as an indication of the unconscious perception of the anxiety-provoking stimulus. It is to be noted, however, that there is nothing in this finding in and of itself that requires such an interpretation. The fact that a subject reports an anxiety-provoking stimulus as seeming clearest more often than the neutral does not require that he has unconsciously recognized the true nature of the stimulus.

Behavior without Awareness to Above-Threshold Cues

While the evidence for discrimination of stimuli that are below levels available to awareness is negative or at best equivocal, there is substantial evidence that behavior can be directed by above-threshold cues of which the subject is unaware. Common sense tells us that we are constantly utilizing cues of which we are unaware in our perception of depth, of shape and size constancy, etc. Experiments have been successful in demonstrating not only the use of cues without awareness but also the lack of awareness in some instances for the responses the subject is making, for the reinforcements controlling responses, and in some cases of the contingent relation between cues, responses, and reinforcement.

Various techniques and experimental arrangements have been used to obtain this behavior without awareness. In some experiments the critical cues are presented at just above threshold intensities or are impoverished by means of a tachistoscope. In other studies the subject's attention is diverted through misdirection in the instructions or the complexity of the experimental task and situation. (In fact the experimental methodologies suggest

that research of this kind might actually have more to do with a concept of attention than one of awareness.) Despite the diversity of experimental techniques for producing behavior without awareness, there has been a lack of systematic investigations determining what factors or experimental variables are necessary in order to elicit this type of behavior. Research of this kind would appear to be a first-order priority.

These experiments on behavior without awareness have used still a different criterion of awareness than what we have encountered in the studies considered above. Here awareness is defined in terms of a series of questions put to the subject, together with his answers. Most typically the subject is questioned concerning his awareness or his ability to verbalize cues he has been using in guiding his behavior, what responses he has been making, and the reasons why he has been making the particular responses. We might note that this method of defining awareness is quite similar to the criteria of awareness frequently used by the clinician. A large part of the clincial conception of the "unconscious" comes from the clinician's observations of the subject's inability to state what it is he is doing, the cues that elicit this particular behavior, and the reinforcement or need gratification that the behavior achieves. In the absence of the patient's ability to verbalize correctly or at least plausibly to questions of this kind, the clinician concludes the presence of unconscious determinants in the behavior.

A definition of awareness in terms of questions asked the subject at the conclusion of the experiment has certain limitations. It runs the risk of involving a memory factor rather than a sheer awareness factor. It is conceivable that a subject may have been aware of a cue or response or a relation between them at the time the behavior actuall occurred, but at the later time when questioned about this, he may have forgotten this relationship or this awareness. Similarily there is the problem of what effect the questioning itself has upon determining awareness. We have noted that in many respects experiments of this kind could

be said to be dealing with the phenomena of attention rather than awareness. Thus when we ask questions of the necessary specificity to be certain of awareness, we are at the same time directing the subject's attention toward the phenomena we are trying to determine. This direction of his attention may be sufficient to give him awareness at this time, whereas at the time the behavior took place he may have lacked awareness.

The definition of awareness in terms of the questions and the subject's answers is only as adequate as are the questions and the answers themselves. Experiments using this criterion must take proper precautions to adequately and precisely specify the questions asked of the subject and his answers. Unfortunately, a considerable amount of the research in this area contains inadequate information of this nature. In many experiments one has to depend solely upon the word of the experimenter that a certain aspect of behavior occurred without awareness. In others it's impossible to determine whether the experimenter means the subject was unaware of the cues, the responses, the reinforcements, or contingent relations between them.

Despite these limitations, the weight of experimental evidence indicates that not only is behavior without awareness a legitimate phenomenon but also that it has been amply demonstrated in the laboratory. But granted the demonstration, what then? The value of such laboratory demonstrations of behavior without awareness for personality theory and dynamics would appear to lie in the opportunity they provide for comparing behavior without awareness with comparable behavior that occurs with awareness. In this manner it would appear possible to verify empirically many of the theoretical distinctions that have been assumed or postulated to exist between conscious and unconscious processes.

While systematic approaches to this problem have not yet appeared, none the less there exists suggestive evidence that would allow us tentatively to compare be-

havior with and without awareness on such criteria as: the complexity of the cues that can be used or the responses that can be acquired without awareness; difference in the degree of transfer and generalization of responses or cues acquired without awareness; and differences in extinction. There is also the interesting problem of personality correlates of proneness to behave without awareness.

Unawareness of Cues

There have been several experiments demonstrating that unaware cues are capable of producing perceptual illusions. Of the series of studies utilizing the Mueller-Lyer Illusion (15, 31, 58) the one by Bressler (8) appears to be the most thorough and best controlled. He found evidence of illusory effects in the judgment of the lengths of the two lines when the directional arrows were so faint that the subjects were unaware of their presence.[3] Brunswi and Herma (9), in what is one of the most convincing experiments in this area, were successful in producing weight illusions in their subjects by ecological associations of color cues with differential weight magnitudes. They found that these cues produced significant illusory effects in the judgment of weights even though the subjects were not aware of any relationship between the color cues and weight magnitude. Their experiment has been successfully repeated by Levin (38), who pointed out the quite interesting finding that the illusory effect produced by the cues was as great in the subjects having insight or awareness as it was in the subjects who were unaware of the cues.

Miller (40) and Perky (44) projected geometric forms on the back of a ground-glass screen at low intensities

[3] The Bressler experiment is frequently interpreted as evidence of discrimination of cues that lie below the threshold of awareness. It is to be noted though that Bressler's own data show that when the thresholds for these lines were determined even the faintest of the lines he utilized were detected significantly better than chance by the subjects as measured by their verbal judgments.

and found that the patterns presented were influential in determining the subject's choice of geometric pattern or the pattern he drew. The effect was found even when the subjects were unaware that a pattern was actually being projected on the screen.

Several investigators have made use of the meta-contrast (2) phenomena as a method of presenting cues in such a way that the subject would be unaware of them. Smith and Henriksson (50) presented a stimulus, consisting of fan-shaped lines, very briefly in a tachistoscope and then, almost immediately after, flashed a square stimulus. While their subjects did not report seeing the fan-shaped lines, they did tend to see the square as a trapezoid. Kolers (34) has presented tachistoscopically sets of designs that required abstraction or conceptualization and found that if cues to the solution of the problems were flashed first, these cues were capable of influencing the type and direction of solution of the problem even though subjects were unaware that the cues had been presented.

Further evidence that cues operating outside of the subject's awareness can influence cognitive problem-solving activity was obtained in an experiment performed in our laboratory. Subjects were presented with two complex geometric nonsense forms side by side under the guise of a concept formation task. In fact, however, the task was an insoluble one, but the right or left position was differentially reinforced. We found that while the position cues were not explicitly recognized in the subject's hypothesis, none the less these cues were influencing the direction the hypotheses were taking.

Complexity of Cues That Can Be Used without Awareness

An interesting but unresolved problem concerns the kinds of cues and their complexity that can be utilized by subjects without awareness. A number of investigators (28, 32, 45, 52) working with concept formation have reported that subjects can correctly designate members of the correct concept class in the absence of adequate ver-

balization of the concept. This would suggest that the subjects are capable of abstracting relationships of a fairly high level without awareness that they are doing so. Unfortunately, these studies are not conclusive. Adams (1) has pointed out that the possibility of correlated hypotheses was not excluded. By correlated hypotheses Adams is referring to the fact that while the subjects' verbalizations of their behavior in these concept-formation studies may not have been correct in terms of the experimenter's definition of the concept, yet their choices may have been consistent with their own verbalizations. Conclusions as to whether subjects can respond to abstract relations without awareness remains to be determined by more careful experimentation that investigates whether or not the subject's choices are consistent with their verbalizations.

Awareness and Acquisition of Responses

As was noted above Levin (38) found that the illusory effect of a cue operating without awareness was as effective as when the subjects were aware of the particular cue. However, there have been few studies that have directly attempted to compare behavior with and without awareness in terms of such criteria as rate of acquisition, generalization and transfer, and extinction.

Several studies have suggested no basic difference in acquisition rate between subjects who acquire awareness and those who do not. Eriksen and Kuethe (21), in an experiment on repression, found that subjects readily learned to avoid shocks by changing their associations to words, and those subjects without awareness learned at the same rate as subjects who were aware of which associations led to shock and therefore had to be avoided. Similar results were obtained in an experiment by Giddan (27) where, under the guise of a brightness discrimination task, subjects acquired a right or left position preference without awareness. The subjects who remained unaware that they had acquired a position preference acquired the habit more rapidly than the subjects who were able to ver-

balize their position preference and the reasons for their acquiring it.

Caution is indicated in generalizing from these two experiments, however. In both, the subjects in the aware group had to acquire their awareness during the acquisition of the response, a factor which would tend to wash out any differences between the aware and the unaware groups in terms of rate of acquisition. It seems reasonable that if subjects had been informed concerning the basis of the correct responses prior to beginning the experimental trials, they would have shown a superiority over the unaware subjects. This advantage of awareness over nonawareness, however, would probably be limited to the type of task involved and the amount of time the subject has available for his responses. Thorndike and Rock (57) and Irwin et al. (33) showed that insight or awareness was not in itself a sufficient condition for perfect performance. Even with insight, subjects required considerable practice in order to achieve a high level of performance.

Awareness and Generalization

There have been several studies that have compared the generalization or transfer of responses acquired with and without awareness. Using the word "good" as a reinforcer, Eriksen, Kuethe, and Sullivan (16) induced response biases for certain nonsense syllables during a pseudo-tachistoscopic task. They found that these response biases acquired without awareness generalized to a pseudo-extrasensory-perception situation as well as to a size-judgment task in which the nonsense syllables were used as categories for the judgments. They also found that the response biases were as prominant on the size-judgment task as in the less structured ESP situation.

In Giddan's experiment it was possible to compare subjects with awareness with those without awareness on generalization. Subjects who had acquired a right or left position habit based upon experimenter reinforcement during a pseudo-brightness-discrimination task were found to

carry their response biases into a new task on concept formation as extensively as subjects who were continued for additional extinction trials on the pseudo-brightness-discrimination task. In addition it was found that subjects who were aware of their position preference and why they had acquired it, generalized the position preference to the new task as extensively as did the unaware subjects.

The failure of Giddan's aware subjects to be more adaptable on the new task is somewhat at variance with our usual conception of the value of insight in adaptive behavior. It points up the need for systematic experimental studies on just how valuable insight is to behavior modification and change and also the need to specify the conditions under which insight contributes. The ability to demonstrate insight or awareness when questioned is no guarantee that this insight will be applied in a concrete situation. Giddan's aware subjects may not have been thinking about the relation of their response biases at the time they participated in the concept-formation task. The possession of awareness in terms of the criteria used in these studies is no guarantee that this awareness will be actively used in the performance of a given task.

Additional information of the effect of awareness upon generalization has been obtained from experiments that have employed "nonvoluntary" responses. Razran (49) reports semantic generalization of conditioned salivary responses and, in addition, noted that "conscious" facilitory or inhibitory attitudes had little effect. Elsewhere (48), he reports that salivary conditioning was much more effective when the subjects were unaware they were being conditioned.

The well-known experiment of Diven's (13) has been frequently interpreted as demonstrating the "unconscious" generalization of anxiety. Subjects chain-associated for 12 seconds to stimulus words. An electric shock was administered upon completion of the associations to the word "barn" which occurred six times on the list. After a five minute rest the list was given again without shock

and the subject's GSRs to the stimulus words were recorded. Although 21 of the subjects in this study were unaware that shock had followed the word "barn" they were reported to have shown conditioned GSRs to this stimulus word. The GSRs also were found to generalize to other rural words.

Lacey and Smith (35) have pointed out certain methodological defects in Diven's experiment, but in a better designed replication of his study, they confirmed Diven's main findings. Instead of GSRs Lacey and Smith used heart rate as a measure of anxiety. They reported that subjects who were unaware of which word was followed by shock not only demonstrated heart rate conditioning but generalized the response to other rural words more than did the aware subjects. In fact the magnitude of conditioned responses was actually greater to the generalization words than to the shock words.

Despite the improvement in design over Diven's study, there are still several defects in the Lacey and Smith experiment that dictate caution in interpreting their results. A possible artifact could have arisen in the following manner. Let's assume that subject number 1 is classed as unaware because he could not state that word a was followed by shock. However, he might be confident that words g, l, j, k, z, etc., were not shocked. This leaves him the remaining words a, b, d, e, etc., to all of which he shows an anxiety reaction. Subject number 2 may be reasonably sure that words b, d, e, k, j, z were not shocked but uncertain about a, l, m, and s. Similarly for subject 3. Each subject can be reasonably sure that some words are not shocked and the words he could exclude would be at least partly nonoverlapping with the exclusions of the other subjects. However, all or nearly all subjects would be unable to exclude the actual shocked word and would show an increase in heart rate to it along with several other random words. Thus when heart rate to words was averaged over subjects, each subject would tend to give a large score to the shocked word as well as to several other words But since the particular other words would vary between

subjects, the shocked word would end up with the greatest mean score.

Just such an artifact could have existed in the Lacey and Smith study, since the data they present are all averages over subjects. To insure that subjects had formed specific anxiety reactions to the shocked words, we would have to examine individual subject records to see if the heart rate was actually greater to shocked words than to any or all other words.

A second question which arises in connection with both the Lacey and Smith and the Diven experiments is the long CS-US interval. The 12-second interval used by Diven and the 15-second interval used by Lacey and Smith are considerably longer than the CS-US interval other experiments have indicated as the optimum for conditioning. This suggested to us that the conditioned stimulus in the Lacey and Smith study as well as in the Diven study was in all likelihood one of the subject's associations to the shock stimulus rather than the shock stimulus itself.

To investigate this possibility we ran a study using the word list of Lacey and Smith and found that subjects showed a very marked tendency to repeat the same associations in roughly the same order on successive presentations of the same stimulus words. In addition we found that the other rural words in the list had a high frequency of occurrence as response words to the rural shock word. On the basis of our results there exists a very strong possibility that in both the Lacey and Smith and the Diven experiments the unaware subjects were actually conditione to one of their own response words, which for many of the subjects was one of the nonshock rural words on the stimulus list. This would account for the generalization obtained and it would also explain why the heart rates to the nonshock rural words in the Lacey and Smith experiment were actually greater in magnitude than to the supposedly conditioned words.

Personality Correlates of Unawareness

There are marked individual differences in the amount and kind of awareness subjects demonstrate in experiments like those we have been considering in this section. These individual differences suggest a fertile field of investigation for the psychologist interested in personality. One familiar with clinical lore cannot help but speculate upon the possible relation between learning without awareness and the clinical conception of the hysteroid personality. The dissociation theory of Janet together with the frequently assumed relation between hysteria, hypnotizability, and automatic writing would suggest that subjects with hysteroid personality characteristics would be more susceptible to behavior modification without their awareness.

The previously described experiment by Eriksen, Kuethe, and Sullivan (16) offers some evidence that this indeed might be the case. Groups of high- and low-scoring subjects on the psychasthenia scale from the MMPI were used in their experiment. No differences were found between the high and the low groups on amount of learning during an experimental session, but there was a very marked and significant difference between the high and the low subjects in terms of awareness. Awareness of their own responses and the experimental events was much more common among subjects in the high group than among those in the low. In view of the previously demonstrated relation between hysteroid characteristics and low scores on the psychasthenia scale (22), these findings would suggest that perhaps differences in levels of awareness would be found to characterize people who were prone to develop hysterical type disorders.

Summary

In this paper I have not tried to provide a definitive survey of the work on the problem of awareness but rather to present a representative sampling for the purpose of evaluation of the concept and to note some of the limitations

of the experimental approaches. One conclusion, however, seems clear. At the present there is no convincing evidence that the human organism can discriminate or differentially respond to external stimuli at a level that is more sensitive or acute than what can be obtained by verbal report. In other words, we have no basis for conceiving of the unconscious as a superdiscriminating system.

Careful consideration of the clinical evidence for the concept of the "unconscious" suggests that an assumption of superdiscriminating powers is not required to account for the observed phenomena upon which the concept is based. Instead, the evidence noted by the clinician or the student of personality would seem to be explainable in terms of either the demonstrable partial independence of different response systems to fragmentary perceptual cues, or lack of awareness for above-threshold cues. We have seen how a nonverbal response can provide above-chance discrimination accuracy in the absence of correct verbal discrimination without the nonverbal response having a lower or more sensitive threshold. When the nature of the stimulation is such as to give rise to a weak or incomplete perceptual process, these fragmentary perceptual cues evoke responses in the various response systems of the individual. Due to the fragmentary nature of the perception, each of the response systems has a large margin of error, but as long as the different response systems are partially independent of one another, these errors are not perfectly correlated. Thus we can expect, for example, that certain emotional responses may be appropriate to the stimulus in the absence of correct verbal responses and vice versa.

The partial independence of different responses of the human organism is not limited to explanations of phenomena that arise in perceptual situations or perceptual recognition experiments. The client whose memory is stirred by the clinician's interpretation may betray the anxiety-laden nature of the memory by certain emotional responses although verbally he is unable to capture the memory.

Here the memory cues are insufficient to insure perfect response accuracy in either the emotional or the verbal response systems, but due to the partial independence of these two systems, better than chance accuracy is obtained in one in the presence of error in the other. Error in the emotional response occurring with a correct verbal response must occur at least as often but usually goes unnoticed either because we do not have a ready-made concept to classify it or because the verbal response itself sets off appropriate emotional responses when it occurs.

The existence of partially independent response systems obviates the need for an omniscient unconscious that is so often implied in the literature on personality dynamics. The present treatment does not require that the "true" memory or the "complete" perception exists somewhere in the unconscious mind.

While not all of the phenomena attributed to "unconscious processes" can be explained in terms of partially independent responses, the current research activity on problems centering around behavior and learning without verbal awareness to above-threshold cues holds forth the promise of shedding light upon the remaining characteristics of the "unconscious." Already research has begun on comparisons of behavior that occurs with and without awareness. From the initial results it would appear that these endeavors will lead not only to increased understanding of behavior dynamics mediated by unaware processes but also to some revisions in the role and importance that we have assigned to language in mediating adaptive behavior. There is a strong possibility that verbal insight may lose some of the exalted status it now enjoys.

A parametric analysis of the experimental situations under which behavior and learning without awareness can take place may well lead to new definitions and approaches to the old problem of attention. In fact there is a strong likelihood that the problem of awareness may be replaced or resolved by a broadened and more precise concept of attention.

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COMMENTS ON PAPERS BY PROFESSORS ERIKSEN AND LITTMAN

by
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My comments shall be brief, first, because there is a large measure of agreement among us, and, secondly, because my paper happens to have dealt with the main areas of disagreement. Littman's historical treatment of motivational phenomena adds significantly to the scope of topics dealt with in these Symposia, and provides an excellent leadoff paper for the final three reviews in this

year's symposium. I find much to agree with in his paper. For instance, I agree that the term "energize" must be used with extreme caution. On the other hand, it is quite clear from my own paper that I disagree that directive and selective functions belong under drive (or motivation). My agreement with Brown in this matter is so much emphasized in my paper that it is hardly necessary for me to repeat the statements here.

There is a further point which seems to require some clarification. While I agree with Littman that a "motivationless" psychology eventually led to sterility in research, I would not agree (and I do not believe that Littman intended to imply) that Fechner's contribution to psychology was less than epoch-making. As a matter of fact, I can think of no better example of the usefulness of psychophysic in present-day psychology than in the ingenious experimental analyses of Eriksen in the present Symposium.

Eriksen's paper is a timely review raising critical questions concerning unconscious processes. Some of the questions that he raised are presently being actively investigated in his own experimental program. I was especially impressed with his finding that substituting lever-positioning for GSR yielded findings similar to those that had previously been obtained with GSR. This is the kind of critical approach to GSR that is very much needed in order to avoid the loose thinking which has led, for example, to the unqualified acceptance of GSR as an "emotional" response.

MEASUREMENT OF DRIVE: AN UNSOLVED PROBLEM IN PSYCHOLOGY

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During the five years that these Symposia on Motivation have been held, some remarkable conceptual advances have been made. From my point of view, one of the most heuristic theoretical contributions was Brown's delimitation of the functional properties of drive, and in particular, his exclusion of directing and steering functions from the definition of "drive" (4). Despite considerable progress, however, it still remains true that some of the most urgent problems facing psychology today are those concerned with the definition and measurement of drive.

What Determines Whether a Stimulus Has Drive Properties?

Excluding directional phenomena from the list of drive properties means that we shall be concerned only with activating (or "energizing") aspects of behavior in our studies of drive. But how can we judge whether a

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given stimulus has activating properties or not? In another important paper, Brown has pointed out that some stimuli, though strong, will not operate as drives. He contends that "a physically intense stimulus tends not to produce a drive if an organism has been exposed on successive occasions to gradually increasing values of that stimulus and if each presentation has been followed by some kind of drive reduction" (5, p. 172). Brown goes on to provide a masterful review of the entire problem of determining how stimuli lose drive properties.

Brown's review makes it very clear that merely because a stimulus is physically intense is no assurance that it will raise the drive level. That is, knowledge of the stimulus is insufficient information concerning its effect on the drive level of any particular organism. As Brown suggests, this is most probably the same point that Skinner (49, p. 374) is making when he says that drive is not a stimulus.

Having precise information concerning the history of the organism, knowing what its experience has been--if any--with respect to the stimulation in question, will permit a better guess than merely knowing the stimulation. If it is known, for example, that the organism has been exposed on successive occasions to gradually increasing values of a particular stimulus, it may be predicted with a high degree of certainty that the drive value of that stimulus will be relatively low.

Brown states that some of the most convincing evidence to support this contention comes from the conditioning studies of Pavlov.

". . . He found that stimuli such as strong electric shocks, wounding of the skin severe enough to draw blood, and cauterization of the skin lost their noxious aspects if, when used as conditioned stimuli for an alimentary response, their intensity was gradually increased. The process of neutralization was apparently quite complete, since according to Pavlov's report '. . . not even the

tiniest and most subtle objective phenomenon usually exhibited by animals under the influence of strong injurious stimuli can be observed in these dogs. No appreciable changes in the pulse or in the respiration occur in these animals. . . .' Incidentally, the fact that the dogs in these experiments were fed for a few seconds after each stimulation appears to be especially significant. Prior to the conditioning training, each of the stimuli apparently had drive value. Marked alterations in respiration and pulse, as well as violent motor reactions directed toward removal of the stimuli or escape from them, were observed. That a complete tolerance was developed for these strong stimuli may be taken as evidence for their loss of drive-arousing capacity. According to Liddell, the major findings in these studies have been amply corroborated in studies at the Cornell Behavior Farm" (5, pp. 172-173).

Note that in the studies of Pavlov and Liddell referred to by Brown, physiological measures were employed to gauge the drive value of stimulation. Drive value was attributed to stimulation prior to adaptation chiefly because rates of heart beat and respiration were observed to increase when the stimulation was applied initially. Moreover, the stimuli were said to have lost drive value when such shifts in level of physiological functioning were no longer observed (after neutralization). While it is true that behavioral observations were also cited, the physiological recordings appeared critical in deciding that repetition of stimulation had reduced the drive value of the stimuli nearly to zero.

Suppose we concentrate for a moment on the problem of how to assess the drive value of a particular stimulus. It is clear from the experiments of Pavlov and Liddell that we cannot rely on intensity of the stimulus in determining its drive value. But suppose we know the past history of the subject and can say with certainty that it has never experienced that stimulus nor any similar one previously; couldn't we then be certain that a strong electric shock, say, would have drive value? In short, wouldn't rearing an animal in isolation, protected from

nearly all forms of stimulation, guarantee that the animal would withdraw from a strong shock?

Curiously enough, however, Melzack and Scott at McGill (39) found absence of reaction to shock in dogs that had been reared under conditions which protected them from most forms of stimulation, including pain. Thus absence of reaction to pain may be produced, as Pavlov and Liddell have shown, by exposure on successive occasions to gradually increasing intensities of painful stimulation. But the McGill experiments have shown that very similar results may be achieved by drastically restricting all forms of stimulation in the early life of the animal.

It would seem, therefore, that even knowledge of the past history of an animal does not guarantee accurate prediction concerning the drive value of a stimulus. If this be true of stimulation from the exteroceptors, how can we be sure that similar difficulties are not being encountered with interoceptors? How can we be sure in experiments which rely on deprivation time to produce a certain change in the state of the organism that such a change is indeed produced with a satisfactory degree of uniformity in all animals?

If we have difficulty even in predicting the effects of procedures designed to change drive level in laboratory animals whose stimulation has been strictly controlled from birth, how much more difficulty must we expect in working with animals reared under ordinary laboratory conditions. And what of all the experiments with human subjects?

There is a second kind of difficulty which arises from our inability to ascertain how various determiners of drive combine to yield the total effective drive strength. Spence has recently stressed this difficulty, indicating that the problem of combining different determiners is exceedingly complex (50, p. 193). Nevertheless, when we depend upon deprivation time, or other antecedent conditions, to gauge strength of drive, we are inevitably faced with the question

of how much that specific controlled condition adds to the total nonspecific level of D. By D, I mean the organism's general drive level to which various drive states are assumed to contribute singly and in combination. This statement is taken from Spence (50, p. 166). I emphasize again, moreover, that D has no steering or directing functions whatever.

In a recent experiment by Franks, deprivation of water, food, and tobacco failed to produce any effect on eyeblink conditioning (16). Should we conclude from such negative findings that learning in human subjects, unlike that in infrahuman species, is not influenced by such deprivations? Isn't it more probable that uncontrolled drive-determinants, in aggregate, were sufficiently strong so that the increment in total drive strength (D) produced by the controlled deprivations was insufficient to affect learning?

Moreover, we should not have to look far to see what these uncontrolled determinants might be. Human subjects approach most experimental situations with considerable uneasiness and apprehension. Isn't it reasonable to suppose that the effect of such fears on the total drive state was so much greater than the effect of the deprivations that the increment from the latter was too small to be detected? The point which I am attempting to make is, I believe, rather close in principle to Weber's Law in psychophysics: the j.n.d. (or delta R) is a function of the general intensity level. [1] That is, under conditions which maintain a high level of drive, a larger increase in drive is required to produce a noticeable difference (e.g., on learning) than under conditions of low

[1] A somewhat analagous suggestion was offered by Peak to account for Court's (8) failure to corroborate her finding that the amplitude of eyelid response is greater when the subject tenses his forearm (42). I realize that it may seem strained to apply the drive concept to forearm tension. I shall have more to say later about such phenomena which seem to be on the fringe of drive.

drive level.[2]

To summarize, there are serious difficulties in the way of gauging drive in experimental subjects, animal as well as human. The difficulties are of two main kinds, (a) our inability to predict the drive value of a stimulating situation, or of a deprivation condition, and (b) our ignorance concerning all of the uncontrolled (and uncontrollable) determiners of total drive (D). Without a doubt we are facing a serious problem. We clearly need better means of assessing levels of drive than any now in common use; and under these circumstances, it seems most imperative to search for new means of gauging drive.

At the present time, in seeking to gauge strength of drive, workers have been relying almost exclusively on certain antecedent conditions in the experimental situation. For example, a six-hour deprivation of food is compared with a twelve-hour deprivation, or a high-intensity shock is compared with a low-intensity shock. Such conditions are antecedent to the present state of the organism.

Instead of always working with antecedents, we might try concomitants for a change. What I have in mind is nicely illustrated by the results of the Pavlov and Liddell experiments referred to earlier. In these experiments the critical evidence deciding whether a stimulus had drive value or not was physiological. When the stimulus produced increased rates of heart beat and breathing it

[2] An alternative explanation of Franks' results could be made with reference to the hypothetical U-shaped relationship between drive and response strength that is discussed in a later section of this paper. According to this view, it is possible to consider that Franks' subjects actually differed in drive, under the two conditions, but that this difference failed to show itself in eyeblink conditioning because the subjects were actually below optimal drive level in the nondeprivation condition and above the optimal level in the deprivation condition.

had drive value. However, following the procedures which destroyed the drive value of the stimulus, application of the stimulus no longer produced the physiological changes.

In this instance the criteria for drive states were not based on the antecedent stimulation but rather on the resulting change in physiological activity. This seems very reasonable, and I believe that everyone would agree that physiological evidence may be used in this way. But I wager that there would be considerable difference of opinion concerning the extent to which we could use such physiological measures as indicants of general drive level. We could expect argument around two main questions: (a) whether physiological measures apply only to situations having "emotional" features, and (b) whether physiological indicants could represent a graded series of drive intensities.

(A) Is Strength of Drive Reflected in Level of Physiological Activity in Situations Other than Those Characterized by "Emotion"?

I believe that very little may be said in answer to this question for lack of data. Physiological methods have, in the main, been applied to the study of "emotion," and the questions which would lead an investigator to secure physiological data on drive, apart from "emotion," have not--as far as I know--been clearly put. For instance, the problem of physiological patterning in emotion has received considerable attention. Is there something specific about the pattern of physiological reactions in fear that distinguishes fear from anger, for example (1)? Now this is a perfectly valid and important scientific question. But emotion has at least two components, an intensive and a directional one. From the work of Cannon and others there is considerable evidence to indicate that whether angry or afraid the animal shows heightened physiological activity (and the results of Ax are in agreement with this). From the directional point of view, however, the states of fear and anger obviously lead to very

different behavior patterns and Ax's results indicate that the physiological patterns differ too. It is clear, though, that our present interest is in the intensive aspect of any state, be it "emotional" or otherwise, and that the study of the difference between fear and anger, for example, is another field of study entirely.

Actually, we should expect deprivation of food, water, sexual gratification, and the like, in raising the general drive state, to increase levels of physiological activity. Appetitional behavior is more conveniently studied with animals, and the simple data which we require here are merely physiological recordings carried out with animals under different conditions of deprivation. Simple and basic as such experiments may seem, however, I am not aware of any such data. Since much physiological work is still methodological in character, it is not surprising that investigators have employed human subjects whose cooperation in wearing various attachments is naturally more easily secured. It may well be, however, that difficulties in recording from animals may be less than is ordinarily supposed.[3] In any event, it will take some time to develop a really satisfactory battery of physiological techniques for work with animals, and it may be that certain measures which are very useful in man may turn out to be not so useful with much smaller animals. It is obvious that this is an area in dire need of experimentation.

Having thus accounted for the dearth of physiological data in deprivation experiments, it is now time to ask whether there are, in fact, any relevant data at all. Actually, the only data to cite, that I know of, are from sleep-deprivation experiments in our laboratory. Admittedly, deprivation of food or water would have furnished a closer parallel to conventional animal studies of drive

[3]Professor David Belanger at the University of Montreal and Mr. Samuel Feldman of our laboratory have found that rats cooperate very well in wearing a pneumograph in a free situation.

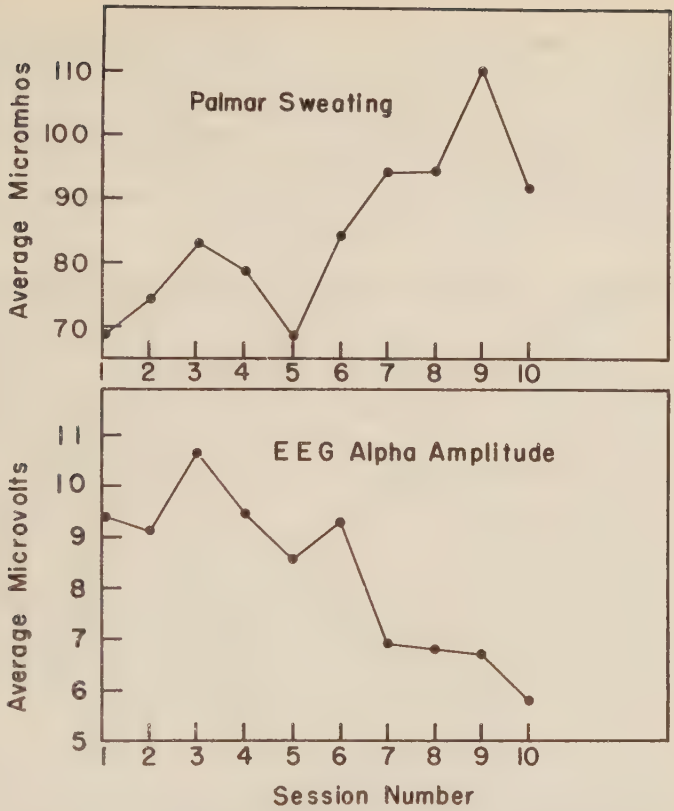

Fig. 1. Levels of EEG alpha and palmar conductance throughout vigils lasting approximately 60 hours. Each point on curves represents average value for one-hour tracking session. Approximate time corresponding to each session number is as follows:-

(1) 11 A.M.	(2) 3 P.M.
(3) 9 P.M.	(4) 3 A.M. (Day 2)
(5) 11 A.M.	(6) 3 P.M.
(7) 9 P.M.	(8) 3 A.M. (Day 3)
(9) 11 A.M.	(10) 3 P.M.

Data based on averages from three young male subjects.

than sleep deprivation. Obviously, such further deprivation studies should be carried out (preferably with animals, as I have just pointed out). But it may be noted that the experiments by Bunch, Frerichs, and Licklider on rats indicated that deprivation of sleep appeared to have very much the same kind of effect upon learning as other forms of deprivation (6).

Physiological Effects of Sleep Deprivation in Human Subject

According to the present hypothesis, sleep deprivation should have the effect of raising the level of physiological activity, reflected in higher palmar conductance, for example. Figure 1 shows that the data supported this hypothesis. The points on the curves represent averaged data from three male subjects who were deprived of sleep for some 60 hours, and who tracked at regular intervals throughout the vigil. The ten tracking sessions were each one hour in length: ten five-minute trials, with one-minute rest intervals between trials. Prior to the vigil all subjects practiced the simple tracking task until no further improvement occurred. Various physiological recordings were made under the standard task conditions.

Note how palmar conductance rose throughout the vigil and how EEG alpha amplitude fell progressively. In order to fully comprehend the EEG data it is essential to refer to a recent study by Stennett (54) working in our laboratory. Figure 2 shows Stennett's plot of the relationship between amplitude of EEG alpha and amount of palmar sweating as measured by electrical recording of conductance with an electrode on the palm of the hand. Note the nonlinear relationship between the two variables. Observe the rise in the curve up to about 75 micromhos on the baseline. Now up to this point, increasing activity of the sweat glands was associated with rising level of alpha amplitude, but beyond this point, as palmar conductance continued to go up, the EEG alpha level commenced to come down.

In other words, the relationship between EEG alpha amplitude and palmar conductance was a curvilinear one.

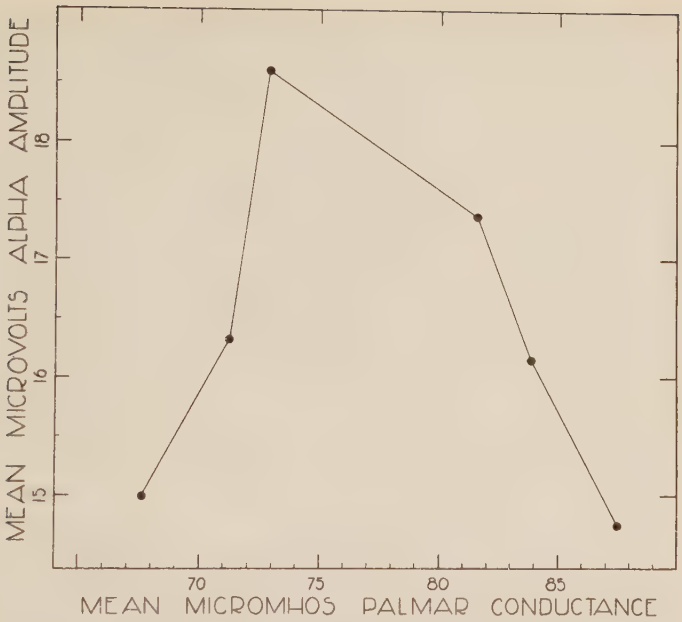

Fig. 2. Relationship of EEG alpha amplitude to the level of palmar conductance. From Stennett (54, p. 136).

This means that EEG alpha amplitude cannot be correctly interpreted unless some measure, such as palmar conductance, is recorded along with it. Falling alpha amplitude accompanied by rising palmar conductance appears to indicate increasing arousal, while, conversely, when it is accompanied by falling palmar conductance, falling alpha seems to indicate exactly the opposite state, namely, decreasing arousal. We must be careful to avoid confusing these findings concerning EEG alpha with the very interesting phenomenon reported by Bjerner (3). This investigator found "depression of alpha activity" associated with lapses (reaction time more than twice the length of the average reaction time) in subjects carrying out serial reaction time performance. Fortunately, Bjerner recorded heart rate at the same time as EEG, and he found falling heart rate associated with these lapses in response. Inasmuch as falling heart rate and falling palmar conductance usually go together, it appears safe to assume that

had Bjerner recorded palmar conductance he would have found falling palmar conductance associated with falling EEG alpha in his experiment. This makes his finding quite different from ours. His is undoubtedly a phenomenon of falling arousal, while ours, of course, is just the opposite. This distinction may perhaps be further clarified by reference to Figure 2 (although no significance is attached to the absolute values). Locate the point for 73 micromhos on the baseline. Now note in moving from 73 micromhos toward zero that the accompanying values for alpha amplitude are falling. This is undoubtedly the part of the curve on which Bjerner's phenomenon occurred. Conversely, our phenomenon seems very well represented by the other side of the curve: going up the micromho scale from 73 to higher values, where the corresponding values for alpha amplitude are falling.

Returning to our experiment, the results from respiration rate fit nicely into the same pattern. The rate was significantly increased in all subjects. Time does not permit detailed consideration of the somewhat more complicated heart-rate and muscle-potential data, although it may be said that these data showed changes which we interpret as being in line with those from EEG, palmar conductance, and respiration.

Looking at all of the physiological data taken during the vigils, the general over-all picture is quite clearly one whi supports the prediction of increased physiological levels. The majority of the peripheral reactions showed increased activation, that is, increased level or rate of response.[4]

Physiological Effects of Incentive and Related Conditions in Human Subjects

Level of muscle tension has repeatedly been shown to be significantly correlated with motivational states. Early

[4]Freeman's experiments on inhibition of micturition appear relevant here. He found "many stepwise increments in palmar conductance" (18, p. 160).

experiments by R. C. Davis are relevant in their indication of a possible correlation between a "build-up" phenomenon in muscle potentials, and the subjects' level of motivation (10). The experimental work of Kennedy and Travis (24, 25, 57) showed clear-cut evidence of a significant relationship between speed of performance and amount of electromyographic (EMG) activity recorded from the forehead. These workers had a very practical interest in the question whether muscle potentials could be used ". . . automatically to warn personnel engaged in monotonous tasks, such as truck driving, before dangerous conditions of inalertness and approaching sleep occur" (24, p. 411).

The physiological approach to motivation in our laboratory also began with electromyographic studies. I have reviewed these experiments elsewhere (33) and I shall, therefore, merely review the main conclusions here. Commencing with Bartoshuk's original formulation of the problem and with his initial positive results (2), there has been a systematic series of experiments which have in all instances yielded positive findings. The first studies were correlational in character, showing that excellence of performance on mirror tracing was significantly correlated with level of tension from various muscles. Such findings were interpreted to mean that degree of motivation was indicated in level of muscle tension.

EMG level as an indicant of motivational strength was found not to be limited to situations involving motor performance. Other situations--such as listening to stories and essays--also yielded findings which could be interpreted in very much the same way. For instance, significant relationships were found between subjects' ratings of degree of interest in listening to stories and essays and the level of tension in facial muscles (58).

Later studies which were designed to study physiological levels as a function of varying incentive, supported the conclusions from the earlier studies. Further experi-

ments showed that physiological measures other than muscle potentials were significantly related to performance (34). The majority of these papers, especially those dealing principally with EMG data, calculated level of activity in terms of steepness of the gradient which was obtained from the beginning of the task until the end. However, since average level correlates very highly with gradient steepness in the case of those physiological functions which clearly show a gradient, and since average level (making use as it does of all of the data) seems a more reliable measure, this measure rather than gradient steepness is mainly used at the present time.

These findings, taken all together, represent strong evidence favoring the conclusion that physiological measures look promising as indicants of motivational strength.[5] Unless it is argued (as some may wish to argue) that threat of punishment is implied in the high-incentive conditions (even when there was no threat of actual punishment by electric shock), we may say that significant positive relationships were found between physiological levels and D, in situations which were mainly "nonemotional" in character. In any event, the Bartoshuk experiments appear to be quite free from aversive conditions, and it appears very difficult indeed to see aversive or emotional conditions in the alertness studies of Kennedy and Travis.

Concept of "arousal" in relation to drive (D). Elsewhere, I have interpreted certain experimental findings from our laboratory in terms of an intensive ("arousal") continuum (33). However, in the present paper, I have considered these same findings within the more conventional framework of drive (D). This shift in terminology requires some explanation. Because the origin of "arousal" is in the context of physiological measurement, it is natura

[5]Up to the present time, our investigations have dealt only with performance, and not with learning. Current physiological experiments in Montreal, however, are directed toward the study of aversive drive in human learning and appetitional drive in animal learning.

to use this term in dealing with phenomena such as palmar conductance and muscle tension. "Drive," on the other hand, has traditionally been inferred from conditions such as hours of deprivation, strength of shock, and other such antecedent conditions. Moreover, until recently, the concept of drive was a compound of direction and arousal. Now that the directional component has been removed, however, it appears that what we have been calling "arousal" comes very close to being identical with the concept of general drive, or D. Furthermore, if physiological measures prove to be generally useful in gauging drive level, it seems that we could drop one of the two terms, and indeed in the interests of parsimony we should do so. In such an event, the symbol, D, would undoubtedly be the term of choice, because of precedent and because the letter is less heavily loaded with distracting connotations than the word "arousal."

Note that I have been able to bring the discussion of arousal this far without once having to use the word "emotion." Remember that in all of this I am talking intensity: purposely excluding direction; and under these circumstances, as Duffy has so convincingly argued (12, 13), it is superfluous to make the distinction between drive and emotion. Furthermore, I do not believe that the concept of emotion is required even in studies dealing with "anxiety," for example in experiments which employ the Manifest Anxiety Scale (55), to study drive. Actually, about one-fourth of the items of the MAS explicitly refer to some physiological function ("I sweat very easily even on cool days," "I do not often notice my heart pounding," and "I am seldom short of breath"), and if mention of "nervousness," disturbed sleep, fatigue, and headache is counted, the proportion of items with physiological connotation rises to nearly one-half. In other words, the MAS, in large measure, serves to grade individuals with respect to their characteristic levels of physiological activity. It is then assumed that high-MAS subjects will come into a learning experiment with higher drive (D) than low-MAS subjects. Now observe that this makes the position of Spence, Taylor, and their associates very similar to

our own. Actually, the main difference seems to be a terminological one: they have been talking about drive and we have been talking about arousal. As far as I can see, there is really no need for the term "emotion" in either case.

There is one more point that I should like to make here with respect to the MAS, and that is, if physiological activity is the key to distinguishing high-D from low-D subjects, then direct physiological measurements would appear preferable to a questionnaire.

Physiological recordings provide continuous measurement all through each experimental session, whereas a sc can merely indicate the probability that a given subject wil be high or low in level of physiological activity. It is obvious that a low-MAS subject, for example, could have a highly "arousing" experience (e.g., a fight with his girl) immediately prior to the experiment. In such a case the prediction from a scale could only be wrong, while actual physiological recordings would be likely to reveal the subject's upset condition. Moreover, continuous tracings enable the experimenter to observe changes during a session (e.g., high activation early in the session, changing over i some subjects to boredom and drowsiness later on). Furth more, for experiments in which subjects must report on several different occasions, it would be useful to have som measure of change in motivational level from day to day.[6]

(B) Is It Likely that Physiological Indicants May Be Used to Provide a Progression of Quantitative Values, Representing a Graded Series of Drive Intensities?

Suppose we continue to find that physiological levels

[6]In a recent unpublished study carried out at the University of Rochester, P. A. Obrist found that measures of palmar conductance significantly differentiated between subjects who met a criterion of conditioning and those who failed to condition, whereas MMPI measures of anxiety failed to differentiate the two groups.

are generally raised with increase in strength of drive. The next question is whether the physiological measures (in some weighted combination) may provide a quantitative means of accurately gauging the continuum of drive (D).

The Freeman experiment. In this instance the reference experiment is Freeman's classic study of reaction time and palmar conductance (17; 59, p. 177). Over a period of many days some 100 paired measures of palmar conductance and reaction time were taken from one subject. The samples were taken on such a large number of occasions and at so many different hours during the day that the subject was certain to be drowsy on some occasions, highly alert (even excited sometimes, presumably) on other occasions; and there were certain to be many stages of alertness between these two extremes. The results from this experiment were exceedingly interesting. When the pairs of values were plotted on two coordinates, a regular-appearing function could be drawn through the points. Low palmar conductances were associated with slow reaction times, high conductances with fast reaction times. The function commenced as a progressively rising one (what would be expected if D were plotted on the base line and response strength on the ordinate). Past a certain optimal range of palmar conductance values though, the curve took a downward turn, so that in totality it had the appearance of an inverted U. We shall return later to a discussion of inverted U curves. However, omitting the descending part of the curve for the moment, the results were very much in line with what would be expected if palmar conductance were indeed a sensitive measure of generalized drive. In other words, palmar conductance appeared to serve in place of conventional conditions (e.g., time since last eating, or drinking) as the independent variable in this experiment.

Obviously an experiment with a single subject needs replication. Schlosberg reports additional positive data (46, p. 85), and recently in our laboratory Stennett performed an experiment which confirmed some of Freeman's

findings (53); but considerably more experimentation is required in this important area.

Experiments on sleep. There is another line of evidence, that from experiments on sleep, which should be mentioned here. Although there are many perplexing problems in connection with sleep, it is safe enough to say that in sleep the general drive level is lower than it is during waking. Factors which increase drive level (such as hunger, thirst, need to void, and "emotion-producing" stimuli), if present in sufficient intensity, will awaken the sleeper. Conversely, a full stomach, a dark, quiet room, and a feeling of tranquility are conducive to sleep.

Now, if physiological measures are to be used as indicants of drive, we should expect these measures to show certain changes as the organism shifts from a waking to a sleeping state. The physiological levels should be lower in sleep than in waking. Evidence on this point, summarized by Kleitman (27), bears out this expectation, which is further supported by recent work from our laboratory. Moreover, from Kleitman's review and from our own data it seems clear that physiological levels are lower during deep sleep (e.g., two hours after onset) than during light sleep (e.g., twenty minutes after onset). Changes in the EEG pattern during sleep are well known. The pertinent EEG findings were recently reviewed by Lindsley (31). Points of reference on his behavioral continuum were deep sleep, light sleep, drowsiness, relaxed wakefulness, alert attentiveness, and strong, excited emotion. Associated with each of these stages along the continuum, rather distinct characteristics of the EEG were described.

Difficulties Related to Quantifying Drive by Means of Physiological Indicants

(a) Failure to distinguish between measures of level and measures of response or reaction. In the case of palmar conductance, in particular, Schlosberg has argued for using level of conductance as measure of the general

state which we have been considering. He says, "The vast bulk of research on electrical skin conductance has lost the forest for the trees. Preoccupation with transient changes, the PGR, has led to general neglect of the slow drifts in absolute level of conductance, despite the fact that the absolute level is the obvious correlate of general level of tension or activation" (46, p. 84).

Schlosberg is making an extremely important methodological point, which applies to nearly all physiological measures, and I can say from our experience that his admonition is highly appropriate. This does not mean, of course, that other important interests may not be served by reaction-type measures. For instance, in Lacey's work on hyperkinetic-impulsive behavior, the reaction-type measure is indispensable (30). But it is essential in avoiding confusion to keep the distinction between level and reaction clearly in mind; and it does appear, as Schlosberg suggests, that for our particular interests, we should concentrate on measurements of level. As a matter of fact, certain misunderstandings have arisen because of failure to distinguish clearly between these two kinds of measures. For instance, some critics have argued that it is impossible to compare measures meaningfully because some measures, like palmar conductance, have relatively long latencies (of the order of one second), whereas other measures, such as EMG, have much shorter latencies (small fractions of a second). Now this argument simply does not make sense when one is using measurements of level. The very nature of a level measure removes it from the category of response with its characteristic of latency. Our measures usually shift gradually (often in an extremely slow drift) from one level to another.

Another fallacy, partly due to the lack of clear distinction between level and reaction, is the notion that every measurable increment in physiological level of activity is by definition an "emotional" reaction. This is the kind of arbitrary classification that Duffy has repeatedly argued against (12, 13).

(b) Lack of standardization in physiological measurement. We face a serious methodological problem in the field of physiological measurement today. Consider palmar conductance, for example, one of the mainstays of psychologists for years. At the present time it is hardly an exaggeration to say that each worker uses his own method which differs in certain significant ways from the methods of others. Level of palmar conductance varies enormously, for instance, with the kind of reference electrodes used and with the resistance of the reference electrodes. With careful preparation of the skin underneath reference electrodes their resistance may be brought down to a constant value (say 500 ohms) in the case of each subject. Without such treatment to the skin there is considerable variation from subject to subject (and from day to day in the same subject) in resistance of the reference electrode. Clearly, high reliability of measurement is impossible when such undesirable variations are present. At the present time it appears that monopolar recording (one electrode on the palm and two reference electrodes over prepared skin of the arm) is preferable for level-type measurements. For reaction-type measurements, however, bipolar (palm-to-palm) recording is widely used. This may be a desirable state of affairs, although I do not believe that there are sufficient data to decide this point. In any event, it seems apparent that in the absence of standardized methodology it is exceedingly difficult to judge the possibilities of a measure like palmar conductance as indicant of drive. The same point may be made with respect to other physiological measures as well.

(c) Individual differences. From Lacey's principle of "autonomic response specificity" (28, 29) and from our principle of "symptom specificity" (35), we expect to find individual differences in patterns of physiological change. We know--for example--that for some individuals heart rate will show more change with increased arousal than palmar conductance, and that for others just the reverse will be true. Schnore's recent data from our laboratory have confirmed this principle of functional specificity (47). He found highly idiosyncratic patterns of

psychological change in individuals, and he also found that these patterns were consistent for individuals in different kinds of situations, and when conditions were such as to produce different levels of physiological activity. These and earlier findings showing physiological specificity make it extremely hazardous to rely on any one physiological measure in attempting to obtain a useful indicant. We obviously need a battery of indicators.

Duffy, whose thorough review of this literature I was privileged to read prior to its publication as a book, recently concluded as follows: "These various forms of responsivenes show, in general, positive intercorrelations, though the coefficients of correlation are apparently not high enough for a measure of any one mode of responsiveness to serve as an adequate measure of the general responsivity of the individual. They appear, however, to give justification to the conception of a responsive or an unresponsive individual, not merely responsive or unresponsive skeletal musculature, skin resistance, or cortical potentials" (13, p. 273).

Low intercorrelations among separate measures have been reported (1), but correlations as high as .50 have been found between muscle tension in the arm and blood pressure in mirror tracing (34). It is clear from the preceding section that some low intercorrelations may in fact be low due to low reliability. It seems very important to stress all of these points in order to avoid the premature conclusions (a) that intercorrelations among physiological measures are invariably low, and (b) that such low intercorrelations rule out the possibility of using physiological measures as indicants of drive.[7]

[7] When a battery of measures is used, it is possible to recognize increased general physiological activation in an individual even though one or two single measures show no appreciable change in him. In fact, we have occasionally observed heart rate in certain individuals to show a change in the direction opposite to that of the battery as a whole. However, such exceptional deviations have not obscured the general trend.

Distinction between Drive Level and General Activity

As Brown indicated, amount of overt skeletal activity does not appear a promising measure of drive (4, p. 4). Rats showing signs of fear (e.g., defecation and urination) in a new situation may be less active than usual, and we know that a man immobolized by terror is enormously more aroused than someone doing his setting-up exercises. Even hunger does not necessarily lead to appreciable increase in "spontaneous" activity (7). It is therefore essential to distinguish physiological indicants of drive from overt skeletal activity.

An ingenious experiment by Richter and Dawson (44) provides a clear example of the kind of distinction which I have in mind. These investigators proceeded carefully through several steps. First, they showed that the lactic acid content of the rat brain was lower in sleep than in ordinary resting conditions. They demonstrated next that exciting the animals by repeatedly removing their support, by allowing them to drop from side to side in a glass beake produced a lactic acid content of the brain which was appreciably higher than that obtained under control condition These results provided strong evidence that lactic acid content of the brain was serving in this experiment as an indicant of general drive state. Since it is known that muscular exercise can cause a rise in the blood lactic aci level, it was important, however, to determine whether the increased lactic acid content of the brain could be merely the result of increased muscular movement which occurred in the high-drive conditions. Further experiments showed, however, that this was not the correct explanation. The higher lactic acid content of the brain recorded immediately following excitement produced by repeated fear stimulation could not be due merely to concomitant muscular activity, because the effect was still observed in animals immobolized by tubocurarine. What is perhaps even more important evidence against the muscle-activity explanation was obtained from further experiments with animals in activity wheels. Animals thoroughl habituated to running the wheels, through long experience,

engaged in extremely vigorous muscular work without producing higher levels of brain lactic acid than control animals that had not been running in the activity wheels. From these and other considerations, the authors concluded that the brain lactic acid was formed in the brain and had not come from the blood.

This experiment furnishes us with a beautiful example of the importance of distinguishing between two superficially similar phenomena which nonetheless are basically quite different one from the other. In these experiments it is clear that lactic acid content of the brain appeared to serve as an indicant of arousal, or D, whereas general activity failed to do so.

Is Response Strength a Monotonic Function of Drive?

According to Hull's theory (with extraneous factors such as inanition excluded), response strength is a monotonic function of D and, to the best of my knowledge, there is wide acceptance of this monotonic relationship among learning theorists today. There is considerable evidence, however, which makes it appear highly likely that the relationship between these two variables is curvilinear (having the shape of an inverted U) rather than being monotonic in character.

The reference experiment in this case is one by Finan (15), who found that equated groups of rats deprived of food for intervals of 1, 12, 24, and 48 hours showed differences in conditioning strength in the Skinner apparatus when conditioning strength was measured in terms of extinction. The optimal deprivation interval was 12 hours. Conditioning strength was less for intervals shorter and longer than the optimal one. Lever-pressing appears to meet the requirements of simplicity and noncompetitiveness of response laid down as conditions for yielding the monotonic function. Moreover, the animals were extinguished at a 24-hour deprivation interval, which, if anything (according to the notion of drive-stimulus sim-

ilarity), should have prolonged extinction for the animals originally conditioned under 24-hour food deprivation.

The Finan data when plotted yield an inverted-U function. What produced the downturn in the curve? If it is postulated that this is the basic relationship between response strength and D, then no further explanation is required. The fact of the matter is that the literature abounds with inverted-U-shaped curves. Hull was well aware of these functions, and indeed, as one step toward dealing with this problem, he postulated an inanition factor. In his Essentials he wrote as follows, ". . . Primar motivation (D), at least that resulting from food privation, consists of two multiplicative components, (1) the drive proper (D') which is an increasing monotonic sigmoid function of h, and (2) a negative or inanition component (ϵ) which is a positively accelerated monotonic function of h decreasing from 1.0 to zero, i.e. $D = D' \times \epsilon$" (22, p. 38).

Another reason why the Finan study is such an apt reference is that the 24-hour deprivation interval which showed decrement in this study does not appear sufficiently long for the appearance of inanition. Actually, physiological criteria for inanition are badly needed in such learning studies. Quimby, Phillips, and White (43) showe that chronic starvation lowered the metabolic rate 10 to 20 per cent below normal in rats. Such positive findings are an encouragement to further studies designed precisel to match the conditions of deprivation in the learning experiments. For example, failure to find reduced basal metabolism at the 24-hour deprivation interval in the Fina experiment would have effectively ruled out the inanition explanation for decrements at that interval. Moreover, the work on human starvation (26) has demonstrated that heart rate, blood pressure, and respiration rate are all lowered in inanition. These findings suggest that a number of physiological measures, in addition to basal metabolic rate, may be applied to this problem.

To review all of the literature dealing with inverted-U functions would require the space of an entire article,

and I shall, therefore, not attempt thorough coverage here. I believe that it will be useful, however, to mention representative experiments to indicate the pervasiveness of this curvilinear function. One of the earliest studies to show the inverted-U-shaped curve was that of Yerkes and Dodson, who found that an electric shock of medium intensity made for fewer errors in learning than did weaker or stronger shocks (60). Courts (9, p. 348) noted that the inverted-U curve which is commonly found in induced tension studies is observed also in situations where strength of incentive is systematically varied.

Spence, in his recent book, presents an inverted-U curve from an experiment by Kaplan, which might very well serve along with the Finan study as a reference experiment for the point which I am attempting to make (50, p. 173). In his study of the relation between rate of bar-pressing by rats in a Skinner box to escape a bright light, Kaplan (23) found an increasing rate of responding up to approximately 180 millilamberts, after which the rate decreased progressively with the higher light intensities. Physiological data should be crucial in experiments such as this one. According to the present view, we should expect physiological levels to rise monotonically with progressive increase of light intensities and to continue rising in the higher range of intensities where performance is declining.

Finally, it should be mentioned that recent reviews by Duffy (13), Eriksen (14), Hebb (20), and Schlosberg (46) have stressed the importance of this curvilinear function. From the historical point of view, it is of interest that Pavlov's writings contained reference to this phenomenon: ". . . For every one of our animals [dogs] there is a maximum stimulus, a limit of harmless functional strain, beyond which begins the intervention of inhibition [the rule of the limit of intensity of excitation]. A stimulus, the intensity of which is beyond that maximum, instantly elicits inhibition, thus distorting the usual rule of the relationship between the magnitude of the effect and the intensity of excitation; a strong stimulus may produce an equal and even a smaller effect than a weak one . . ." (41, p. 213).

Response competition as explanation for decrement in performance with increased D. Farber, Montague, Spence, Taylor, and their colleagues (40, 50, 51, 52, 56) initiated a fruitful program of studies with human subjects, in which the Manifest Anxiety Scale has been used to separate "high-D" from "low-D" subjects. On theoretical grounds these workers have been predicting for relatively simple, relatively noncompetitive learning situations that high-D subjects will excel over low-D subjects. Actually, as the Spence, Farber, and McFann study shows, even fairly complex verbal learning in human subjects does not exceed the critical level of complexity at which reversal occurs. In their study the high-D learner were superior to the low-D ones (51).

Some of the conditions under which the reversal is expected to occur (i.e., shift from superiority of high-D to low-D performers) have been very clearly stated by Spence, Taylor, and Ketchel: "Under conditions in which the habit strength of the correct response is weaker than one or more competing responses, a higher D level would be expected to result in poorer performance. This implication follows from the assumption that D will multiply the habit strengths of both the correct and incorrect responses, thus increasing the amount by which the excitatory strength (E) of any stronger competing response will exceed that of the correct response. Since performance is assumed to be a function of the magnitude of the difference between the excitatory potentials of the correct and incorrect responses, it is obvious that the higher the level of D the greater will be the advantage of the incorrect responses and hence the greater the likelihood of the occurrence of such erroneous responses" (52, p. 309).

On the whole, the evidence has supported these theoretical predictions rather well. In the studies of conditioning and extinction, the "high-anxiety" groups consistently responded at a higher level than groups from the low end of the Scale. Moreover, as previously mentioned, "high-anxiety" subjects even excelled in relatively complex rote learning. But in learning more difficult lists

especially designed to increase response competition, there was the predicted reversal from high-D superiority to low-D superiority.

However, it appears that Spence and his colleagues may have overlooked an important variable in these experiments. In rote-learning studies, the competitive lists, in addition to providing more opportunities for response competition, are more difficult than the lists with word pairs of high association values. Now finding that with increasing difficulty of tasks there is a corresponding increase in accompanying muscle potentials (11, 48) suggests that level of D may be in part a function of task difficulty. Although it may seem strained to speak of task difficulty increasing drive, the physiological findings certainly suggest this possibility. It therefore appears that in designing an experiment to test the response competition hypothesis, these workers may have inadvertently overlooked (and thus failed to control) variation in D from condition to condition.

What I am suggesting, of course, is that it is possible to interpret the rote-learning data rather differently from the authors' explanation. With the effect of task difficulty on D unknown and uncontrolled in these experiments, it is possible that the results were produced simply by rise in D directly, rather than by greater relative potency of competing erroneous responses. In other words, it seems possible that the correct responses were suffering from too much D rather than that they were losing out because of competing responses being strengthened by the greater D.[8] What this comes down to, obviously, is the notion

[8]This statement would also apply, it should be noted, where the correct response is strongest, but where competing responses that are subthreshold with low drive are moved above threshold by raising drive. According to Taylor (56, p. 305), higher drive can reduce the level of correct performance under these conditions. In other words, I am suggesting that we consider an explanation of the data in terms of task difficulty as an alternative to any form of the response-competition hypothesis.

that beyond some optimal point, D weakens rather than strengthens response.

Fortunately, an experiment by Sarason provides exactly the kind of data that we need (45). To test the hypothesis which I suggest as an alternative to the response-competition hypothesis of Spence and his co-workers, one might proceed in the following way. Select a rote-learning list on which high-MAS subjects have been shown to excel over low-MAS subjects, under a given set of incentive conditions. Now, if the present view is correct, it should be possible to produce a reversal of the kind that the Iowa group have obtained, by raising incentive (increasing D), instead of introducing different lists designed to increase the number of competing response tendencies. That is, by raising incentive to a critical value where low-D subjects were near the optimal point on the hypothetical inverted-U curve and where high-D subjects were beyond this point, we should obtain better performance from the low-D (low-MAS) subjects than from the high-MAS subject

As previously mentioned, Sarason designed his experiment precisely along these lines. Actually, in his experiments, there were three groups of subjects, low, medium and high on the MAS. Sarason found "that high motivation instructions were detrimental for high-anxious groups and facilitating for low- and middle-anxious groups."[9]

Sarason's results cannot be explained in the way that Spence, Taylor, and Ketchel accounted for their findings (52). Assuming that correct responses were stronger than incorrect ones, and with the knowledge that the high-MAS subjects excelled over the low-MAS subjects with

[9]In a number of related articles, employing the MAS, Matarazzo and co-workers have published inverted-U curves of the kind that would be predicted according to the present hypothesis. Curves having inverted-U shape were obtained for time (though not trials) to learn a stylus maze (37), for performance on a pursuitmeter (38), and for digit symbol learning (36).

this particular learning material, explanation in terms of competing responses would surely lead to the prediction that with increased D, high-MAS subjects would continue to excel. This prediction, of course, would be directly contrary to Sarason's findings. Now Taylor believes that increased drive can lower performance level even if the strongest response happens to be the correct one providing competing responses which are subthreshold with low drive are brought above threshold by raising the level of drive (56, p. 305). It would appear that Sarason's results might be accounted for along these lines. But Hill has presented strong arguments against the logic of such an explanation (21), suggesting, however, that it might be possible to devise a more complicated assumption about response competition which would predict that the low-MAS group would excel.

While it is permissible, of course, to go on increasing the complexity and number of explanations required in order to retain the postulate of monotonicity, the number and complexity of factors needed for this purpose seem to be growing, and I wonder whether we may not have reached the point where it would be more economical to revise the postulate.

Neuropsychological considerations. In last year's Symposium, Lindsley reviewed evidence concerning the role of the reticular formation in the mediation of drive (32). Earlier, Hebb had put forward the hypothesis that drive (or arousal) varies as a function of the amount of cortical bombardment by impulses from the reticular system (20). In proposing the inverted-U curve to describe the relationship between level of arousal function and level of cue function, Hebb suggested, quite reasonably, that the decrement in cue function at the high-arousal end might be due to the facilitation of conflicting responses. In line with present arguments, an alternative neural explanation for the downturn in the curve is suggested. For purposes of exposition let us consider a simple response, say bar-pressing. Such a response is presumably mediated by neurons organized in phase sequences (19). Now, as Hebb

says, a certain level of nonspecific bombardment is required for such phase sequences to run off smoothly. However, it appears quite conceivable that there is an optimal level of bombardment which if exceeded will prevent the required neural activity, say by making certain critical neurons refractory when it is their turn to fire in the sequence. Such internal breakdown within the neural mechanism for bar-pressing itself could account for decrement in the response, and it would therefore be unnecessary to introduce the explanation of interference produced by other competing responses.

Summary

One of the most serious problems facing psychology today is that of providing satisfactory criteria for defining and quantifying drive. Up to the present time such criteria have been sought almost exclusively in antecedent conditions, such as intensity of stimulation in aversive situations, and hours of deprivation in appetitional ones. However, serious difficulties have been encountered in attempting to use these antecedent conditions to define the strength of drive.

It is impossible to gauge strength of drive from the intensity of stimulation. Habituation combined with positive reinforcement may completely remove the effectiveness of even highly painful stimulation. Yet protection of the animal from painful stimulation does not insure that it will react aversively to a painful stimulus. Dogs raised in isolation with drastically reduced stimulation, including pain, showed a striking absence of reaction to noxious stimulation.

Moreover, there are serious problems associated with the use of hours of deprivation as a measure of drive. Zero time of deprivation does not, of course, mean zero drive. There are always uncontrolled determinants of drive combining to produce an unknown level of drive. But how can we ever obtain measures of all these deter-

minants which must vary considerably from individual to individual in a group of animals, and from one experiment to another?

In short, the difficulties encountered in attempting to use such antecedent conditions in gauging drive strength appear to be insuperable. Under these circumstances we are forced to seek in other directions for satisfactory means for measuring drive level. Instead of continuing to depend solely on antecedent conditions, it seems high time that we tried concomitants for a change.

In the present paper reasons are given for trying physiological measures as indicants of drive (D). As a matter of fact, physiological measures had been used to show that formerly effective pain stimulation had lost its drive-properties. Such indicants have at least one decided advantage over all measures based on antecedent conditions. They offer the possibility of measuring the combined effects of all the various and sundry antecedent conditions which, at any moment in time, determine the level of drive (D). The point I should like to stress is that physiological measures are not being offered as handy tools for ancillary purposes. They are not being proposed just to give us fuller information, or to add data concerning "emotional factors" in the drive state. I maintain that we are forced to find some satisfactory means of measuring drive, because none exists in psychology today. Under these circumstances it is not merely desirable to search along new lines but it is absolutely necessary to do so.

Present experimental evidence, while insufficient, is nonetheless encouraging with respect to quantifying drive by means of physiological measures. What we now require are more experiments to supply data which are urgently needed.

Finally, there is evidence which appears to cast strong doubt on the assumption that the relationship between respons strength and D is in fact a monotonic one. Arguments for a curvilinear relationship between these variables are advanced

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COMMENTS ON THE PAPERS BY PROFESSORS ERIKSEN AND MALMO

by
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I hope it is not out of order to open these comments by referring to my own paper. In Part IV, Section I, under "Convenience" I discussed the possibility of R-S

laws with the implication that such laws neither existed nor would be of any use if they did. I am wrong on both counts, for there does exist at least one clear-cut instance of such laws and it is an instance which plays a crucial role in a contemporary theory. In the various versions of Hull's system there has always been a place made for a device to give continuity to behavior in the absence of external stimulus conditions which might be correlated with behavior changes and to explain the way in which goal attainment influences behavior earlier in the sequence. This device consists of the relations between the fractional antedating goal response -- r_G -- and the fractional antedating goal stimulus -- s_G. In running down an alley, for example, some of the movements which an animal engages in are preparatory consummatory ones. Stemming from these movements or preparatory responses is a characteristic stimulus, response induced, which then functions to initiate responses which are conditioned to it, and so on to the end of the behavior sequence. Now the point of this is that what Hull has done is to postulate a set of laws of the form $r_G \rightarrow s_G$ and in great measure his ability to explain continuous, chained behavior rests upon such response-stimulus laws. So R-S laws do exist, and far from making one's mind reel (as I suggested in my paper), they make excellent sense.

Now to Eriksen and Malmo. First there is a methodological item to which I would like to draw your attention. That is the use to which they have put correlational methods. [1] It has been many years since "experimental" psychologists have used the inherent power of correlation methods, especially partial correlation, to help make sense out of their investigations. There was a time when, for example, the student of the maze and of maze learning

[1] After returning from the Symposium I read Cronbach's Presidential Address. If I had attended the APA meetings at which it was delivered I should have made even more of a point of it at the Symposium. Now that his address is published, there is no reason for me to labor a point which he has made in such a thorough manner.

considered problems which were essentially of a correlational sort. But as interests changed and experimental procedures began to find their own statistics, correlational procedures in experimental psychology diminished in importance. But there are inferential needs in scientific work that cannot be completely satisfied by manipulatory designs, and we have in the work of Eriksen and Malmo two very powerful testimonials to this. I think they have taken an important step by showing how theoretical and experimental reasoning in psychology may proceed more freely by using the correlation as a weapon of analysis.

I am quite persuaded by Eriksen's analysis that there is no supersensitive unconscious discrimination system. It is especially welcome to see the problem of subthreshold discrimination analyzed properly, for once, in terms of the statistical properties of thresholds--probability distributions rather than points. Nevertheless, I think it should be emphasized that he has demonstrated that there are responses of people which are correlated with stimulus conditions independently of the correlations which exist between such stimulus conditions and the verbal behavior of people. The fact that such correlations are of a lower order of magnitude than ones in which verbal items participate is a matter of efficiency, so to speak, a functional problem of considerable applied importance, but one which should not be permitted to mask the theoretical implications of such relations. Eriksen has given us a much sounder analysis of a set of phenomena about which many people have spoken in quite bizarre ways and it would be unfortunate if his remarks were taken to mean that he has scotched a belief rather than made it more sensible.

In general, I am inclined to go along with most of what Eriksen has said. There are several questions, however, that I would like to raise. In each of the cases where he has contrasted discrimination with awareness (verbal response) with some other "indicator" response, there has been only one such indicator. As a result his

partial correlation model has so far been tested only for the relationship between verbal behavior and some other kind of response system. (For reasons which I shall indicate briefly below, I do not feel that it is proper to treat the subjective confidence procedures as representing a quite different system from other kinds of verbal behavior elicited from subjects.) It would be very instructive, therefore, to see what happens when two or three nonverbal response systems are simultaneously compared with verbal responses. I might say that it will be difficult to find such systems; GSR as well as the electrical resistance level itself are probably not verbalizable-about by the subject, while other systems, such as pulse, respiration, and eyeblink are. But suppose they are found, would we find that for each of these the partials would differ significantly from all others?

One shouldn't do such a thing in public, but I am inclined to predict that they wouldn't, that all nonverbal response systems would be perfectly correlated with one another (save for error factors and the consideration discussed in the next paragraph) and that they would all have the same low order of relationship to the verbal indicators. Another way of putting it is that there will be no significant increment in the multiple R if, to a correlation between some verbal and nonverbal response pattern, we add still another nonverbal response indicator.

On the other hand, I would venture that whenever verbal instructions are involved which the subject must interpret and whenever there is a response which the subject can correlate with linguistic activity, my prediction would not hold. I think that language has some sort of monitoring effect upon the organism's own behavior and that it is variations in the power of such monitoring that generates the partial correlations Eriksen has found. Therefore, I would predict that effector activities with respect to which the subject has no linguistic control--he cannot say, "GSR, go down," but he can say, "Move that lever two inches"--would all be perfectly correlated with one another but imperfectly with linguistically controlled effector ac-

tivities, and that the linguistically controlled effector activities would all be imperfectly correlated with one another by virtue of their different histories of language growth and training. As a concrete example, if we are to believe some of the feats attributed to Yogis, it seems likely that their patterns would be different from those of the Western college sophomore, because the Yogi has brought under linguistic control activities which ordinarily are not.

I find myself very much in sympathy with Eriksen's dissatisfaction about having to equate awareness with verbal response. However, I am not at all sure that it is the only working hypothesis. Before I am accused of some fancy sort of "gamesmanship," I want to say that I don't mean to imply that I have a way out of the problem. I am baffled by the same sorts of things that baffle Eriksen. I would like to suggest, however, that some of the trouble lies in the way that he regards language. He calls it a response system and implies, if I may judge from his model of a branching relationship of different response systems to a prior perceptual process, that language responses should be treated like any other. I believe that this is a bad hypothesis. Linguistic behavior is not only a far more complex thing than the movement of articulatory organs--the analogue of GSR, pulse, or eyeblink--it is actually a different kind of thing. For example, it has already built into it a discriminative component so that one simply cannot refer to linguistic behavior without implying some discrimination, as one can in dealing with GSR, pulse or eyeblink, though, to be sure, a causal analysis of these latter concepts will require reference to discriminative activities. Therefore, to posit a discrimination process without verbalization and then compare it with one which does entail verbalization is to imply that we have done no more than substitute a verbal response for a nonverbal one. And I don't think this is true at all.

It should be pointed out that I don't believe this is a bad hypothesis just because the data happen to turn out a certain way. There are many independent grounds for

regarding linguistic behavior as having a special relationship to all other behavioral systems of the organism. Some of the reasons are phylogenetic and some are developmental. In any event, for some of the reasons that Eriksen himself lists, it does not seem wise to equate awareness with the presence of verbal responses. For example, we attribute awareness to some kinds of organisms but not to others, we attribute it to preverbal children after a certain age, we attribute it to certain sorts of brain-injured persons but not others, and so on. In all these cases, we use nonlinguistic behavior--here I would include sounds made by the organism--functionally to designate some condition we call awareness; of course we also use language behavior in much the same way. But since we do not rely exclusively upon verbal behavior and even reject its "evidence" at times, e.g., suspected malingerers, hysterics, etc., there seems no compelling reason to regard language responses as uniquely related to awareness. What I am saying here, as in my first point, is that I don't believe that an organism's language behavior all has the same methodological status as effector responses yielded by, e.g., the vocal organs, heart, etc.

I would like to comment upon the model that Eriksen has proposed in still another way. For one thing, I don't like his choice of words. The term "perceptual" process is not neutral enough and ordinarily is not broad enough to cover all the things he must intend by it. I think that something like "cognitive" process would be preferable. To my way of thinking at any rate, "cognitive" is a more neutral term while it is also a good deal more general. I know that "perception" has in recent years come more and more to take on this flavor, so my preference here is perhaps nothing more than just that--a preference. For another thing, the term "perceptual process" cannot refer to anything very specific. Much like the term "motivation," "perceptual process" is a category or class term and hence merely designates visual, auditory, gustatory, etc., processes. Unless one wishes to extend quite markedly what vision, audition, and similar processes are like, I should think it would be preferable to indicate

the complex processes to which I believe Eriksen is referring by some other set of words. It seems to me very questionable to assume that because some of the things that happen in relation to vision and audition, say, are very much alike, that therefore vision and audition are themselves very complex processes and, indeed, the whole of what might be postulated to intervene between stimulation and responding.

Finally, we keep coming back to the crucial notion of awareness. I confess that I hold an old-fashioned, indeed reactionary and heretical, view of it, for our times. I think of it as something quite like a "raw feel"; unlike the psychologists of the past century I believe that raw feels are subject to enormous alterations and variations phylogenetically and developmentally, so that there is no fixedness for any given physicalistically defined situation unless one defines such a situation as including the laws describing the conditions of raw feels. If the perceptual (read, cognitive) process that Eriksen proposes includes the possibility of what I refer to when I speak of raw feels, then I am very much inclined to take the bull by the horns and say that the dangers of the egocentric predicament are no greater than those of the naive behavioristic predicament. In other words, I believe that when a subject is giving a verbal report he literally is reporting or describing and is not just saying "ouch." While we do not know very well how he comes to make such utterances, I see no reason why his utterances under, say, laboratory conditions should be regarded as having any different methodological status from the utterances of the experimenter. Both individuals are engaging in descriptive activities (for the occasions with which we are concerned), though ordinarily the experimenter has a good deal more information at his disposal.

Now I don't want to get into a philosophical dispute at this time, but I do think it is essential to separate out from the rest of his behavior all linguistic behavior of the subject that might be regarded as descriptive, I know that this runs counter to the thesis of some of the logical

positivists that the "social" language of the subject and experimenter should be differentiated from the experimenter's scientific metalanguage. In general I accept that position, but I do differ as to what is to be called metalanguage and what is not.

I am making a point of this matter in relation to the study of awareness because I think we have gotten to the stage where it is silly to continue insisting that the only psychological "existences" are stimuli, effector responses, and their correlations. I believe we are finally ready to admit once again something like "mental contents" or "raw feels" into psychology without crippling ourselves. And I am willing to argue that "awareness"--or some of the things we refer to when we use that word--is just exactly an instance of a raw feel or mental content. I might say that I have been strongly encouraged along these lines by the recent retreats from the absurd position of radical behaviorism that people like Carnap and others once held (and to which I too adhered, though there is nothing especially noteworthy in the fact that my beliefs have changed).

Awareness, then, can be regarded as a datum of psychology for which we once again have the task of determining the characteristics and conditions. If we recognize this candidly and do not feel so guilty about the way we have berated our predecessors that our analysis and research suffer, then I think our understanding of perception will expand enormously. In this development, the distinctive relation between language and awareness will emerge and, I hope, we will be able to distinguish usefully between the descriptive and nondescriptive functions of subject language. When this happens, awareness will no longer have to be identified with verbal activity; at the same time, I believe we will find even stronger reasons to believe that language development and capacity is a powerful determinant of what the properties of awareness may be like and how they may be altered.

I expect to see Eriksen in the forefront of this movement because I take his conclusion that "A parametric

analysis of the experimental situations under which behavior and learning without awareness can take place may well lead to new definitions and approaches to the old problem of attention [and to the] strong likelihood that the problem of awareness may be replaced or resolved by a broadened and more precise concept of attention" as a promise that he will be doing just that research. And to judge from what he has said at the Symposium and the research he has done, I don't think we can find a better man.

Malmo and I are not going to be in very good agreement when it comes to the conventions that we urge on our colleagues. He takes the energistic orientation toward motivation adopted by all good Yale men, first or second generation. I would urge for him as I did for Brown and Farber that what motivation means to most psychologists is not the same thing as "energizing," in their sense, at least, and I would urge that he not insist upon this because he will lose the chance to make clear the positive virtues of his proposals. For while I disagree about the generality of what he calls drive, I am very favorable toward his analysis of drive in relation to the sorts of theoretical and methodological questions with which he is concerned.

Malmo has hit very hard at one of the institutionalized kinds of laziness to which psychologists are prone. Just as some of us prefer to use a test or questionnaire to find out about some kind of behavior instead of going out to observe it, so there are many of us who prefer to define drive in terms of some readily conducted set of operations instead of directly observing whether the organism is in the condition that the operations are supposed to induce. Now, there is no reason why we learning psychologists cannot attempt to draw laws relating environmental deprivation or stimulation variations to behavior changes; this is surely one of the eventual goals of applied psychology But in the case of drive, these variations are used to characterize the state of the organism. In that event, it seems to me essential to determine whether the animal

has actually gotten into that state. The fact that regular relations between some environmental manipulations and a dependent behavior variable can be observed is, at best, only a necessary condition for believing that we have actually placed the organism in a particular state. What we have failed to do is carry out the remainder of the program, viz., finding out what are the laws relating our environmental operations to the state of the organism. If this is not done, then we are going to get all sorts of things called "drive" simply because the dependent behavior follows a certain form when this is not at all what we mean to do. What we intend to do is relate some state of the animal to the dependent behavior. It will not do at this point to say that the state I have been talking about is really just a hypothetical one and that therefore it can be handled any way one wishes; I think that once one goes to the trouble of inventing a concept to help him explain something, he then has an obligation to work with it in its own terms, not as a disguised law--as in the present sort of situation--relating environmental conditions to behavior conditions. So I feel that Malmo's program is very promising because it enables us to move beyond the traditional kinds of defining operations for drive which we know are defective (see, for example, his discussion of the effects of experience with stimuli in relation to drive); in addition, it permits us to have a rational basis for knowing when such operations may be expected to have similar consequences, as well as giving us a basis for relating such conditions to the multitude of endogenous conditions which also alter behavior.

Granting all this, there remains the problem of response strength in relation to drive. Malmo is concerned about the fact that in Hull's system there is a postulated monotonic relation between D and response strength when an enormous amount of research indicates clearly that as D increases, response strength varies in a roughly inverted U-shaped manner. I think he is correct in saying that such an unadorned hypothesis will not do. But there are a number of alternatives in trying to develop a theory within the Hullian framework that will correctly render

the observed nonmonotonic relationship between D and response strength. Malmo has suggested that the remedy is in reformulating the definition of D so that it is nonlinearly related to response strength; that is a good way. Another way, however, is to make the assumption that D always interacts with still other variables to influence response strength and that therefore the influence of D alone is always some function of the values for other parameters. This is the method chosen by Spence, for example. While I think Spence has made a dreadful mistake in using something like the MAS to index D (Malmo's insight that much of the MAS refers to physiological phenomena is, by the way, a terribly important one, I believe), the use of K and V permits him to account for the observed curvilinearity relating D and response strength. It should be noted that, as usual, I am arguing a logical point here rather than an empirical one, and I don't necessarily imply that I am therefore especially favorable toward the choice that Spence has made. I'm still not sure.

I have the feeling that Malmo's conceptions are more likely to be found fruitful in the area of personality, especially that portion that deals with temperament and perception, than in the area of learning. For example, intuitively, I can see how his notions of alertness or arousal may be tied in with Eriksen's desire for a revivification of the concept of attention. Also, our views of learning are becoming so diverse and different from those current even ten years ago, e.g., mathematical theories, that I should hesitate to bind an arousal or alertness theory of drive--indeed, almost any theory of drive or motivation--to extant formulations of how learning occurs. Furthermore, the sorts of learning theories to which Malmo's arousal notion might be attached do not yet deal with sufficiently complex behavior situations to permit the full scope of these notions to be explored. But this is much more likely to be the case at another level, that of the functioning organism in the kinds of settings that clinicians, social psychologists, and students of personality (including the factorists) investigate. Finally, since it is evident that learning theory is rapidly being changed from some-

thing which is primarily focussed on the H factor into something more general in which H plays an increasingly minor role, it may not be too long before Malmo's proposals will be exploited by those who once regarded themselves as learning theorists (I'm talking about the future). And in such a setting they will carry a good deal more weight.

COMMENTS ON THE PAPERS BY PROFESSORS LITTMAN AND MALMO

by
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I was greatly relieved when Professor Littman's paper preceded my own at this symposium since I had considerable guilt feeling over presenting a paper dealing with unconscious processes at a gathering devoted to shedding light upon the confused topic of motivation. I had a number of rationalizations ready which were designed to show that my topic was in fact pertinent to the problem of motivation, but when confronted with Littman's definition, I find them unnecessary. Since processes are actives and therefore motivational, apologies are not required.

I am impressed with the scholarly background and breadth of viewpoint that Littman has brought to the field of motivation, and in particular with his emphasis upon the effect of our own cultural linguistic heritage upon the concepts we choose as scientists. While most of us are aware (and certainly the semanticists have been) that the discriminations we emphasize and the concepts we favor are culturally determined, it is not often that this awareness is explicitly applied to current psychological concepts in as penetrating a manner as Littman has done.

I find myself in general agreement with the main tenor of Littman's thesis, particularly with respect to the course of action his analysis suggests. As I understand it, the

various parties to the controversy over what is motivation should continue with their respective empirical approaches, attempting to systematize the facts in their particular areas. However, they shouldn't insist too strongly that their approach is motivation or all of motivation. This is, of course, eminently reasonable but I strongly suspect that scientists will continue to be people first and scientists second and that Littman's analysis of the motivational problem, no matter its validity, will have little mitigating effect upon the arguments over what motivation is or includes.

And perhaps this is for the best since argument is not only fun in and of itself, but it contributes to scientific advance through the testing of ideas and sharpening up of concepts. Also I must confess that while intellectually persuaded by Littman's thesis, I still can not shake the feeling that with more data most of the phenomena that most of us consider motivation will yield to over-all systematization. But this feeling is probably a reflection of my lack of emancipation from my cultural conditioning.

Perhaps it is this same animal faith that causes me to find Professor Malmo's position quite congenial. While Littman, I am sure, will find Malmo's conception of drive too general and ambitious, I do not feel that this is a serious objection. In one sense at least, Malmo's approach would be consistent with Littman's analysis. By defining drive in terms of energization or arousal and excluding directive or steering functions, Malmo is in effect trying to work with one of the separable characteristics that have been lumped into the grab-bag of motivation. While this is certainly going to exclude much of what some people call motivation, Littman's analysis suggests that this state of affairs is inevitable. No particular problem should arise as long as the term "drive" is used and we are careful not to claim that this is all there is to motivation.

I am especially impressed with the divorcement of physiological symptoms from emotion that Malmo has made. There is increasing evidence that we have been

too ready to associate the physiological symptoms of "anxiety" with the emotion of fear. Instead it would appear that much of what we term anxiety is really a high level of drive or arousal that can occur with strong approach "motivation" as well as in avoidance situations. Our everyday usage of the term certainly reflects this since we describe ourselves and others as being anxious when anticipating an imminent pleasant event. A good example is the behavior of a child on Christmas Eve.

It may be that a concept of drive solely in terms of activation will not be as generally useful as Malmo and others hope, but this is only to be decided empirically. The chances for drive proving a useful concept in the area of motivation will be vastly increased if Malmo's attempt to define the drive state in terms of physiological measures proves feasible. Such an outcome would free the conception of drive from definitions in terms of antecedent conditions and permit the systematic study of the stimuli that arouse drive.

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